PHYSICS

OF THE

EARTH'S CRUST.

PHYSICS

OF THE

EARTH'S CRUST

BY

THE REV. OSMOND FISHER, M.A., F.G.S.,

RECTOR OF HARLTON, HON. FELLOW OF KING'S COLLEGE, LONDON, AND LATE FELLOW AND TUTOR OF JESUS COLLEGE, CAMBRIDGE.

SECOND EDITION, ALTERED AND ENLARGED.

London:

MACMILLAN AND CO.

AND NEW YORK.

1889

First Edition, 1881.
Second Edition, 1889.

Cambridge:

PRINTED BY C. J. CLAY, M.A. AND SONS,

AT THE UNIVERSITY PRESS.

PREFACE TO THE FIRST EDITION.

For many years past I have been convinced that various questions of Physical Geology might be answered negatively, if not positively, by applying to them simple mathematical reasoning, and quantitative treatment. My own views have, in some respects, been greatly altered by the application of this method; and I confess that the present work contains within itself evidence of the circumstance; for not only will it be found that the views now put forward differ in some respects from those which I have previously published in contributions to scientific periodicals; but the effect of the progressive application of the quantitative method may be traced in the book itself; and the development of ideas, proposed in the earlier chapters, will sometimes be found to have taken an unexpected turn later on. This remark applies especially to certain hypotheses, which at first presented themselves in a favourable light, to account for compression and for the formation of ocean basins.

It is extremely probable that, if these investigations are carried further, some of the theories now offered as fairly established, may turn out to be untenable. With a growing science like Geology this is unavoidable. On a review of what I have written, I feel how many difficulties have been left unsolved, and some which have occurred to me not even mentioned. Nevertheless I hope that this attempt will not be without a certain value in advancing the study of the Physics of the Earth's Crust.

The mathematical reader will perhaps be surprised by the rough and ready mode of treatment adopted in some instances. But when it is recollected that, for the most part, we can assign only very hypothetical values for our symbols, it would be affectation to seek close results, which would after all have no greater value than those which claim to be only distant approximations.

The course of the argument and the general conclusions, will be found repeated in the Summary. But it is hoped that a perusal of this may not be substituted for reading the book. It is believed that the reasoning of the several chapters will be found intelligible, even without wading through the calculations.

My thanks are due to the Councils of the Geological Society of London, and of the Philosophical Society of Cambridge, for permission to make free use of papers contributed by me to their publications. I have similar acknowledgments to make to the Proprietors of the *Philosophical Magazine*, the *Geological Magazine*, and the *Quarterly Journal of Science*. My kind friends, H. W. Bristow, Esq., F.R.S., Senior Director of the *Geological Survey*, and A. F. Griffith, Esq., B.A., of Lincoln's Inn, Scholar of Christ's College, Cambridge, have rendered me most valuable help in revising the proofs while they were passing through the press.

OSMOND FISHER.

Harlton Rectory,
18 *October*, 1881.

PREFACE TO THE SECOND EDITION.

My reasons for bringing out a second edition of this work before the first is out of print are these. Possible explanations of some of the difficulties left unsolved in the first edition have since occurred to me. Investigations have also been carried on by others, which appear to strengthen and support some of the conclusions already arrived at; these needed to be followed up and embodied. Lastly, advancing years warn me that if I am to publish again it must be done quickly, while health and strength are granted. In doing this, I feel not a little encouraged by the approving reference to the first edition made by Professor Judd, then President of the Geological Society, when in 1887 he presented to me in the name of the Council the balance of the proceeds of the Lyell fund.

A great part of the book has been rewritten, and while there are many additions there are some omissions. Some portions have been omitted because they seem uncalled for in the present state of Geological opinion; and some because they would not have accorded with the results arrived at in the new portions. At the same time these results appear to be the legitimate deductions from generally received geological data, now for the

first time submitted to mathematical treatment. The most important additions will be found in Chapters V, VI, VIII, IX, XV, XVII, XVIII, XX, XXIII, XXV.

My old and valued friend H. W. Bristow, Esq., F.R.S., had revised the proof sheets of more than half the volume, as he had formerly done for all the first edition, when to my great sorrow he was attacked by serious illness. I have had the good fortune to secure the assistance of John Brill, Esq., M.A., Fellow of St John's College, Cambridge, who is an accomplished mathematician: and he has verified all my calculations, and rectified errors, so that I feel great confidence in the general correctness of the work. I am also in particular indebted to him for the ingenious analytical treatment of a problem, which occurs on pp. 240 to 245.

The Summary has been made rather more full than in the former edition, so that it gives a less inadequate epitome of the work.

OSMOND FISHER.

Harlton Rectory,
11 *June*, 1889.

ERRATA.

Page 50, line 8. *For* "lakes" *read* "cakes."
" 94. *For* line 16, *read*
$\frac{d\phi}{dt}$ = the rate of the fall of temperature of the shell at the level of no strain.
" 107, line 6. *For* "they" *read* "it".

CONTENTS.

CHAPTER I.

ON UNDERGROUND TEMPERATURE.

CHAPTER II.

INTERNAL DENSITIES AND PRESSURES.

CHAPTER III.

CONDITION OF THE INTERIOR.

CHAPTER IV.

CHANGE OF DENSITY ON SOLIDIFICATION.

CHAPTER V.

A LIQUID SUBSTRATUM DISSOLVING GAS ACCORDING TO HENRY'S LAW MAY ACCOUNT FOR THE ABSENCE OF TIDES AT THE EXTERIOR SURFACE OF THE CRUST.

CHAPTER VI.

A THIN CRUST IMPLIES AN ENERGETIC SUBSTRATUM.

CHAPTER VII.

LATERAL STRESSES AND RESULTING INEQUALITIES OF SURFACE.

CHAPTER XI.

LIQUID SUBSTRATUM.

CHAPTER XII.

CRUST NOT FLEXIBLE.

CHAPTER XIII.

DISTURBED TRACT.

CHAPTER XIV.

THE REVELATIONS OF THE PLUMB-LINE.

CHAPTER XV.

THE REVELATIONS OF THE PENDULUM.

CHAPTER XVI.

THE REVELATIONS OF THE THERMOMETER.

CHAPTER XVII.

THE SUB-OCEANIC CRUST.

CHAPTER XXI.

VOLCANIC DYKES.

CHAPTER XXII.

THE VOLCANO IN ERUPTION.

CHAPTER XXIII.

GEOLOGICAL MOVEMENTS EXPLAINED.

CHAPTER XXIV.

GEOGRAPHICAL DISTRIBUTION OF VOLCANOS.

CHAPTER XXV.

A SPECULATION ON THE ORIGIN OF OCEAN BASINS.

SUMMARY.

[The Roman numerals refer to the Chapters.]

CHAPTER I.

ON UNDERGROUND TEMPERATURE.

The rise of temperature on descending into the earth—The causes which have been assigned for it—Attempts to determine the law of increase by observation—Practical difficulties—Variations in temperature rate partly real—Flow of heat constant—Conductivity.—Its effect upon the rate—Rose Bridge Colliery.—Deep bore-hole at Sperenberg, and methods of observation used there—General effects of convection currents in a bore-hole—Tabulated observations at Sperenberg—Anomalous result supposed to be deducible therefrom—Anomaly explained—General law of average rise of temperature—Mean rate determined by Prof. Prestwich—Hot springs and volcanos prove that the rise of temperature continues, but do not inform us whether the law near the surface obtains unaltered for all depths—Presumption that it does not.

THERE is no fact more firmly established in terrestrial physics than that the temperature of the rocks at the earth's surface increases with increasing depth. Observations upon this subject have been made in all parts of the world, and a similar result has been everywhere arrived at. Even in the frozen soil of Yakoutzk (or Jakutsk) in Siberia, this increase of temperature is found to prevail, although the ground is congealed to the whole depth penetrated[1]; the limit of frozen soil being 620 feet.

[1] This increase proves that the freezing is owing to the present low mean temperature of the locality, which Prof. Milne states to be $-8{\cdot}6$ R. or $12{\cdot}7$ Fahr. ("Geol. Mag." N.S. vol. IV. p. 518). If it were a residual effect of a former still colder period in glacial times, the strata would be now warmer above than below, whereas the reverse is the case.

The temperature at the depth of 20 feet is $13{\cdot}6^0$, at 50 feet $17{\cdot}1^0$, at 300 feet, $25{\cdot}0^0$, at 381 feet $26{\cdot}6^0$: mean increase 1^0 for 28 feet.

It is found that in high latitudes the soil is frozen to greater depths under

In mining operations this gradual increase of temperature becomes a very serious consideration, rendering labour at great depths a very severe trial to the constitution of the workman. For when the air has to be conveyed to long distances it acquires the temperature of the rocks, and no means have been yet suggested which could furnish it at the atmospheric temperature unaffected—or but slightly so—by its long journey through the subterranean passages.

This increase of temperature, though universal, is not everywhere the same. The average is about 1 degree Fahr. for 50 feet of descent. Such is the result of very numerous observations. Some of these have been made by drilling holes in the rock in deep mines; others by lowering thermometers in artesian bore-holes. A committee was appointed by the British Association to report upon the subject, and their reports extend from the year 1869. Much information upon the matter may also be gathered from the Reports of the Parliamentary Commission upon the Coal Supply.

Unquestioned as is the fact, nevertheless the cause of this increase of heat has formed the ground for much speculation. The most obvious explanation of it is offered by the phenomena of hot springs and volcanos. These show us that, in some places at any rate, much higher temperatures exist at great depths than have ever been reached by artificial perforations. But these phenomena might be said to be local, while the general slow increase of temperature we have been describing exists more or less at every place. Are these phenomena directly connected? Are they indirectly connected? Or, are they altogether unconnected? Various answers have been returned to these questions.

Among "practical men," engaged in mining operations, an opinion seems to have prevailed that the increase of temperature in deep mines is due to the pressure of the overlying strata or "cover." We may unhesitatingly dismiss this hypothesis. Pressure by itself cannot develop heat. That takes place only

valleys than under high ground, owing to the stiller atmosphere and longer duration of cold. (Brit. Assoc. Report upon the Depth of Permanently Frozen Soil in the Polar Regions, 1886. Also "Nature," vol. xxxiv. p. 485.)

where some kind of motion, which however slow may be termed 'visible,' is arrested and transformed into that invisible motion of the ultimate molecules of a body which constitutes heat.

In deep coal-mines an effect called "creep" is caused by the enormous pressure upon the sides of a passage, or upon the pillars left to support the roof, causing the floor of the excavation to swell up. In this case there is immense friction between the particles of the rock, were it not for which the passage would become instantaneously closed. It has been remarked that considerable heat sometimes accompanies this creep, in which, be it observed, we have not pressure alone, but motion destroyed by friction and converted into heat.

Sir Humphry Davy ascribed the volcanic fires to the oxidation of earthy and metallic bases, supposed by him to exist low down in the interior of the earth. Since water is everywhere present in the crust, if it be dissipated by the abstraction of its oxygen when it encounters the bases, and by the blowing-off from volcanic vents of the equivalent hydrogen, then its place must be continuously supplied by the percolation downwards of fresh accessions; and thus a continual production of heat was supposed to arise, and to manifest itself by its escape towards the surface. But "Sir Humphry Davy himself renounced his bold chemical hypothesis in the last volume which he published—'Consolations in Travel, and the Last Days of a Philosopher[1].'"

A very fascinating theory, which at first met with support among many scientific men, was promulgated by Mr Mallet, who considered the heat of volcanic action to be derived from the transformed work of the crushing of the rocks of the earth's crust, owing to the contraction of its interior through long-sustained cooling. The cause of evolution of the volcanic heat under this theory was of the same kind as that alluded to above, in the case of creeps; while the ubiquitous increase of temperature in descending into the earth was looked upon as chiefly a manifestation of the generally diffused heat of the interior, by the escape of which the contraction in volume is produced.

[1] Humboldt's "Cosmos," vol. I. p. 25. Sabine's Transln. London, 1845.

Mr Mallet supposed volcanic action to be caused by this contraction manifesting itself locally and paroxysmally.

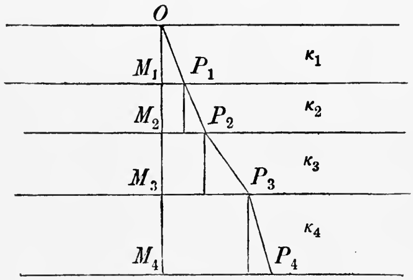
In order to decide what theory best accounts for the phenomena, it is obvious that the first step is to make sure of the facts themselves. In other words—What is the law regulating this increase of heat? At what rate does the temperature augment? This might be supposed to be a point easily settled. It might be supposed that nothing would be easier than to insert thermometers into the rock at different depths in a mine, and to read off their indications; or to lower them into the bore-hole of an artesian well for the same purpose. But there are many difficulties to be overcome, and a host of disturbing causes present, which differ according to the circumstances. In the case of a mine, the rock, or the face of the coal, is affected by the temperature of the air. The air is warmed by the presence of men and horses; it is cooled by the ventilation carefully kept up. Hence the surface of the working is not at the true temperature of the rock. The result of careful experiments made by Sir G. Elliot showed that when thermometers were inserted in the coal in a long-wall working, at distances of 3, 6, and 12 feet from the face, no alteration could be detected in the indications given beyond 3 feet from the face of the coal; and his observations were accordingly taken at 4 feet. It is evident that the necessary distance must be governed by the difference between the temperature of the ventilating air and of the seam, and that the greater this difference the farther it would be necessary to bore into the coal to obtain a near estimate of its true temperature. If again it be attempted to ascertain the law of increase by means of an artesian bore-hole, there are here also disturbing causes present. The action of the tool warms the rock by mechanical means, the friction or pounding action developing considerable heat. Hence while the work is in progress no reliable observations can be taken, except during intervals of suspension, and then it appears—as will be seen further on—that the interval needs to consist of weeks rather than of days. When the work is completed, and the bore stands nearly full of water, it is the temperature of the water which is obtained on lowering a thermometer into it;

this is affected by convection currents, and we cannot be sure that it coincides with the temperature of the rock. In fact many reasons may be assigned why it should not do so. Springs may enter on one side and flow out at the other; or they may rise from lower levels and flow away at higher. The very act of lowering the thermometer tends to mix up the differently heated layers of water, and to confuse the result.

In spite of these sources of error of observation, it is by no means necessary to suppose that variations of rate are not to a considerable extent real, and arise from varying heat-conducting power in the strata. The flow of heat from below upwards at any given locality must be practically the same at all depths penetrated, for suppose M_1, M_2, to be any two points, of which M_1 is the nearer to the surface. If the flow of heat which enters through M_2 were greater than that which escapes through M_1, the strata between M_1 and M_2 would grow continually hotter, instead of remaining practically at fixed temperatures. And if the reverse were the case, they would grow cooler. The flow must therefore continue the same at all depths, for all times embraced by human history; though not necessarily so for geological periods. This being premised, we may perceive the effect, which variations in conductivity[1] will produce.

[1] The conductivity of a substance is measured by the quantity of heat that flows in the unit of time through unit area of an infinite plate of the substance of unit thickness, owing to its opposite faces being maintained at temperatures differing by one degree. The numerical value of the conductivity therefore depends upon (1) the nature of the substance, (2) the unit of length, (3) the unit of temperature, and (4) the unit of heat. The ratios of the conductivity of different substances will not depend upon the arbitrary units chosen, so long as reference is made to the same units for all the substances. Now of the two methods commonly used for measuring heat one, the "calorimetric," takes as the unit the amount of heat required to raise unit mass of water to one degree higher temperature; and the other, the "thermometric," takes the amount of heat required to raise unit volume of the substance under consideration to one degree higher temperature (Maxwell's "Heat," p. 255, 1877). It is evident that, when we wish to bring the conductivities of different substances into comparison, we must use the same unit measure of heat for them all; which will require the same standard substance to be used, viz. water. The corresponding numerical value of the conductivity is usually expressed by k. Here κ is used, because k is reserved in this work for another quantity. The unit of heat used in the thermometric measure of conductivity being generally smaller, because the

Suppose F to be the flow of heat, which, from what has been said, will be constant, κ the conductivity and v the temperature



at the depth x. Then these quantities are connected by the equation

$$F = \kappa \frac{dv}{dx},$$

where $\frac{dv}{dx}$ is the temperature rate, or increase of temperature per foot of descent. Since therefore

$$\frac{dv}{dx} = \frac{F}{\kappa},$$

the temperature rate will vary inversely as the conductivity, and therefore must increase proportionally, where the heat-conducting power of the rock is less, and *vice versâ*.

Suppose O to be at the surface, and OM_1, M_1M_2, M_2M_3, M_3M_4, to be strata, whose conductivities are κ_1, κ_2, κ_3, κ_4, respectively. And let M_1P_1, M_2P_2, M_3P_3, M_4P_4, represent the excess of temperature at the successive depths over that at the surface. It is evident from the figure that $\frac{dv}{dx}$, the temperature rate (or

capacity for heat of water is greater, a larger number of them will express the same flow in the same interval. Consequently the numerical value will be greater. The symbol commonly used for it is $\frac{k}{c}$, where c is the capacity for heat of the substance as compared with water. The thermometric measure would be inapplicable to the case before us, because the unit of conductivity would not be the same for the several strata, and they could not be compared together.

gradient), is the tangent of the angle which the line $OP_1P_2P_3P_4$ makes with the vertical within the several strata. Accordingly it appears that, if observations are taken at underground stations within the same stratum, the increase of temperature per increase of depth will give the true rate at these stations. But if observations are taken in different strata, the thickness of the strata between the stations becomes an element in the mean result. Thus if R_4 be the mean rate for the whole depth OM_4; r_1, r_2, r_3, r_4, the rates within the several strata; x_1, x_2, x_3, x_4, their thicknesses; the diagram shows that

$$R_4 = \frac{x_1r_1 + x_2r_2 + x_3r_3 + x_4r_4}{x_1 + x_2 + x_3 + x_4}.$$

The conductivities of various kinds of rock differ so much, that very great variations in the temperature rate may be ascribed to that cause alone. To take as an instance Rose Bridge Colliery; the temperatures were taken there by drilling a hole a yard deep at the bottom of the shaft during the process of sinking. If the mean temperature of the surface there be taken at 50° F., the mean rate of increase for the whole depth was 1 degree for 55 feet. But at the successive depths of 605, 630, 663, 671, 679, 734, 745, 761, 775, 783, 800, 806, 815 yards respectively the rate appears to have been 1 degree for 70, 25, 49, 24, 24, 110, 66, 32, 42, 48, 51, 36, 54 feet. Here the highest rate was 1° in 24 feet, and the lowest 1° in 110 feet; the former being nearly four times the latter. In the list of conductivities given in the foot-note[1], it appears that the highest (that of sandstone in Craigleith quarry) is thirteen times that of the lowest (coal); both of which kinds of rock would occur in a coal-mine.

The low value of the conductivity of ice may probably explain the high temperature rate, viz. 1° for 28 feet, in the frozen soil of Yakoutzk.

We will next consider the kind of circumstances which affect the observations made in an artesian well, taking as an instance the deep boring carried down at Sperenberg, near Berlin. This

[1] Conductivities of rocks from Everett's "Units and Physical Constants," pp. 101, 103; 1879. The units used are the centimetre, gramme, and second

reached the extraordinary depth of 4052 Rhenish feet, or 4172 British feet. A most surprising geological fact about this boring is that, with the exception of the first 283 feet, which were carried through gypsum with some anhydrite, the remainder passed entirely through rock salt. This seemed to offer an exceptionally good opportunity for observing temperatures in a homogeneous rock, which might be expected to be comparatively free from the sources of error arising from variable heat-conducting power in the layers successively penetrated. The observations in the bore were taken with much precaution, under the direction of Herr Eduard Dunker, Inspector of Mines[1].

(C. G. S.). The values below are those of κ to these units; and are arranged in order of magnitude.

Coal (mean)	·00080
White sand (dry)	·00093
Calcareous sandstone	·00211
Sand of experimental garden (Edinburgh)	·00262
Clay slate (Devonshire)	·00272
Clay with $\frac{1}{4}$ its weight of water	·00310
Slate across cleavage	·00337
Trap rock of Calton Hill	·00415
Caen stone	·00433
Cornish serpentine	·00441
Micaceous flagstone across cleavage	·00441
Limestones (mean)	·00515
Granite (mean)	·00530
Sandstone and hard grit (dry)	·00545
Sandstone and hard grit (wet)	·00590
Slate along the cleavage (mean)	·00600
Micaceous flagstone along the cleavage	·00632
White sand saturated with water	·00700
Siliceous sandstone (slightly wet) mean	·00747
Sandstone of Craigleith quarry	·01068

To these may be added the conductivity of ice, which along the axis is ·00223, and perpendicular to the axis ·00213.

It might perhaps be possible to determine the conductivities of certain strata at different depths in a mine *in situ*, and also the rates in them. The product of the two in each case would express the flow of heat at that depth. If this was found to be the same at each depth it would afford a strong presumption that the determinations had been correctly made. The flows of heat at different geographical localities could then be compared, instead of comparing the temperature-rates.

[1] Dunker assumed a formula to express the law of increase, which in fact represented a parabola having its axis horizontal. Mohr, finding that this

This bore-hole, from the favourable circumstances already referred to, and the care with which the observations were made, deserves full consideration. The temperatures were taken in two manners. In one set of observations the temperature of the water in the bore-hole was observed with a suitable thermometer; in the other an apparatus called a geo-thermometer was lowered, which cut off a portion of water from that above and below it by two disks or bags. A thermometer was enclosed in the space between the disks, and, after the thermometer had been down not less than ten hours, the temperature shown by it was assumed to be that of the rock at the corresponding depth. This was found to differ sometimes by more than a degree Réaumur from the temperature of the water at the same depth, as shown by the first-named thermometer, when the water above and below was not cut off. Now, it can hardly be supposed that so great a difference as this really existed simultaneously between the surface of the rock exposed to the water in the bore-hole and the water itself. On the contrary, it seems evident that the surface of the rock, being exposed to the action of the water, must have assumed the temperature of the water, whatever that might be, at any given depth. But the water was undoubtedly affected by convection currents, and consequently its temperature was not the same as that of the rock *in mass* at a distance from the bore-hole. Hence, when the circulation of these currents was stopped by the disks, the surface of the bore-hole would begin to tend towards the true rock temperature. If the water at any depth was warmer than the rock in mass, its temperature would begin to fall when the currents were cut off, and if the water was cooler than the rock it would begin to rise.

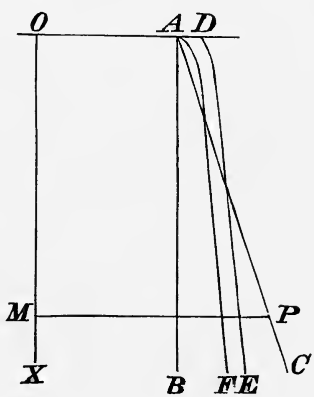
Let us, then, shortly consider the general effect of convection currents as they would affect the water.

These currents arise from the circumstance that when water is warmed it expands, and consequently becomes lighter. This

curve became vertical at the depth of 5170 feet, erroneously concluded that the increase of temperature would come to an end at that depth; and that the source of heat was therefore to be sought within the crust itself. (Ninth Report of the British Assoc. on Underground Temperature, 1877.)

expansion is exceedingly small; yet in a substance of such extreme mobility as water it is sufficient to cause the expanded portions to rise, while the denser portions above sink to supply their place. The expanded portions in rising carry their heat up with them, and the cooler portions in sinking reduce the temperature of the lower part of the column. If therefore we had a column of water originally warmed towards its lower portion, but not supplied there with fresh accessions of heat, the whole would, if open to the atmosphere, shortly assume an equable temperature throughout.

A diagram will render the subject more clear. Suppose the depths in the bore-hole to be measured along the vertical line *OX*, and let lines drawn at right angles to this represent in proportion to their lengths the temperatures at the corresponding depths. Let *OA* represent the mean temperature of the surface of the ground. Then on the supposition that the temperature of the rock in mass increases proportionally to the increase of depth, the temperature of the rock will be represented by a *straight* line, as *AC*. If, then, the temperature of the water in the bore-hole correctly gave the temperature of



the rock, its temperature would be likewise represented by the same straight line *AC*. But since it is affected by convection currents this cannot be the case; for they tend to warm the upper portions of the column of water, and to cool the lower.

Hence, so far as we have gone, the water in the upper part of the bore-hole will be warmer than the body of the rock, and in the lower part it will be cooler; and in our diagram we must draw a line to represent its temperature, more distant from OX in the upper part, and nearer to it in the lower. It need not, however, be a straight line, the law of increase of temperature being possibly altered. Suppose, then, DE to be this line, or the curve of temperature of the water on the supposition now made. It will be seen that it must intersect the line AC.

There is a further consideration to be taken into account. If the bore-hole is nearly full of water, and of considerable dimensions, it will present a considerable surface to the atmosphere. At Sperenberg the water stood seven feet from the stage of the bore-pit, and the bore-hole was a foot in diameter. The consequence would be that the open surface of the water would be cooled, sensibly to the temperature of the air. This would bring the curve of temperature of the water at the surface nearly to the same point as the temperature line of the rock there, and it would also have the effect of reducing the temperature of the water throughout the column, below what it would be if it had not this extrinsic cause of cooling. The ultimate result would be that the temperature curve of the water would assume some such form and position as AF. It will be observed that this line also intersects AC; and the signification of this is, that the water in the bore-hole is warmer than the rock mass in its upper portion, and cooler than the rock mass in its lower portion; or, in other words, the water tends to warm the rock in the upper portion, and to cool it in its lower portion. Consequently, if the circulation of the currents be interrupted, the water imprisoned between the disks of the geo-thermometer would in the upper portion of the hole gradually become cooler, as heat was conducted away from it into the body of the rock; and it would gradually become warmer in the lower portion, as heat was conducted into it from the body of the rock. But at the particular depth at which the two lines AC and AF intersect no alteration would take place.

That such an effect was actually produced is shown by the following table, the first two columns of which are taken from

Dunker's Table II., in his paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers, und die desshalb in dem Borloche I zu Sperenberg auf Steinsalz angestellten Beobachtungen." The two temperatures marked with asterisks differ from those quoted in the British Association Report from Dunker's quarto paper of 1872 in the "Zeitschrift für Berg-Hütten und Salinen-Wisen." But they are printed as here given in the octavo paper from which they have been copied, and are stated there to be the mean results of several observations. Dunker considered the deepest observation with the geo-thermometer unsatisfactory.

The difference between a Prussian and an English foot is so small that, for the purpose in hand, they may be considered equal.

I	II	III	IV	V	VI	VII	VIII	IX
Depth in feet.	Temp. R. Water shut off.	Temp. R. Water not shut off.	Water shut off, consequent alteration of Temp.	Surface Temp. 7·18°. Absolute Increase at each depth in Col. II.	Being at the rate of 1° R. per No. of feet.	Absolute Increase at depths.	Being at the rate of 1° R. per ft.	1° F. per ft.
15	9·40	10·35	−0·95	—	—	—	—	—
30	9·56	10·20	−0·64	—	—	—	—	—
50	9·86	10·40	−0·54	—	—	—	—	—
100	10·16	12·30	−2·14	2·98	33	—	—	—
300	14·60	13·52	+1·08	4·44	45	—	—	—
400	14·80	14·30	+0·50	0·20	500	—	—	—
500	15·16	14·68	+0·48	0·36	277	—	—	—
700	17·06	16·08	+0·98	1·90	105	—	—	—
900	18·50	17·18	+1·32	0·44	455	—	—	—
1100	*19·90	19·08	+0·82	1·40	143	11·72	94	42
1300	21·10	20·38	+0·72	1·20	166	—	—	—
1500	22·80	22·08	+0·72	1·70	118	—	—	—
1700	24·10	22·90	+1·20	1·30	154	—	—	—
1900	25·90	24·80	+1·10	1·80	111	—	—	—
2100	*27·70	26·80	+0·90	1·80	111	7·80	128	57
2300	28·50	28·10	+0·40	0·80	250	—	—	—
2500	29·70	29·50	+0·20	1·20	166	—	—	—
2700	30·50	30·30	+0·20	0·80	250	2·80	214	95
2900	—	31·60	—	—	—	—	—	—
3100	—	32·70	—	—	—	—	—	—
3300	—	33·60	—	—	—	—	—	—
3390	36·15	34·10	+2·05	5·65	122	—	—	—
3500	—	34·70	—	—	—	—	—	—
3700	—	35·80	—	—	—	—	—	—
3900	—	36·60	—	—	—	—	—	—
4042	38·25	38·10	+0·15	2·10	310	7·75	212	95

In the above table the first column gives the depths in Prussian feet, at which the observations of temperature were made.

The second column gives the temperatures observed at the respective depths when the convection currents were stopped by the use of the geo-thermometer.

The third column gives the temperature of the water in the bore-hole when the convection currents were allowed to circulate.

The fourth column gives the alteration of temperature at each depth which arose from stopping the convection currents—*minus* when the temperature fell, *plus* when it rose.

The fifth column gives the increase of temperature at each depth over that at the previous one, as taken from the second column.

The sixth column gives the rate of increase at each depth $\left(i.e.\ \frac{dv}{dx}\right)$, measured by the number of feet which it would be required to descend to gain an increase of 1 degree Réaumur, on the supposition that the increase per foot remained constant for that number of feet; consequently large numbers show a proportionately slow increase.

The seventh column, like the fifth, shows the increase of temperature at the depth against which the number stands, over that at the depth at which the previous number stands; the object of this column being to obtain averages at longer distances apart.

The eighth column shows the rate of increase of temperature with longer averages than the sixth, measured by the number of feet of descent for 1° Réaumur.

The ninth gives the same increase, when measured in feet of descent for 1° Fahr.

In discussing this table the first point to be noticed is that the rate of increase is by no means so equable as, from the homogeneity of the rock, it might have been expected to have been. In order to obtain anything like a general law of increase it is necessary to take the average of the increase at considerable distances apart.

The second point has been already adverted to, viz. that the shutting-off of the convection currents caused a diminution of temperature in the upper part of the bore-hole, and an

increase of it in the lower. This shows clearly that the temperature of the rock was altered temporarily, by the action of the convection currents, to some distance away laterally from the bore-hole. The hole was lined with three tubings, one behind another, for the upper 440 feet, and on account of the presumed convection currents still existing behind the tubing, the observations in the upper part of the column were not considered trustworthy. But this circumstance does not militate against the conclusion at which we have arrived, because whatever effect convection currents might have upon the temperature would be *pro tanto* produced by these concealed currents, so that all they could do would be to lessen the effect of shutting off the currents in the main channel. And we may reasonably suppose that the change of temperature arising from that proceeding would have been still greater than recorded if the tubing had been in close contact with the rock.

In the various discussions of these observations[1] the observations made with the geo-thermometer have been in general looked upon as giving a near approximation to the temperature gradient of the earth's crust at this locality; and, consequently, it is these which we propose to consider further. Although the sixth column shows that the rate of increase of temperature was far from equable when comparatively short intervals are taken into account, yet, when we pass to the eighth and ninth columns, we observe that on the whole there is a decided diminution of the rate of increase in the lower depths. It was probably this circumstance which induced M. Dunker to assume that empirical formula for the law of increase which led Prof. Mohr to believe that, at the depth of 5170 feet, the increase would be *nil*, and thence to conclude that the source of the heat of the crust must be situated within the crust itself, instead of the heat—as is usually supposed—coming up from the profound depths below. The question which presents itself therefore (and it is a most important question) is—Can this

[1] The writer has met with a course of lectures entitled "Vorträge über Geologie, von F. Henrich, Wiesbaden, 1877," in which the Sperenberg observations are discussed with much acumen.

diminution of the rate of increase in the indications of the geo-thermometer be consistent with an equable rate of increase in descending as deep as the observations went into the earth's crust? The answer, that it can, seems to follow from the considerations already made on the effect of convection currents upon the temperature, not of the water only, but of the rock itself. It is obvious that the continued contact of water at a different temperature from that of the rock must alter the temperature of the rock itself where it is in contact with the water. And it has been remarked that, almost beyond dispute, it had actually that effect; consequently the rock in contact with the water must have been cooled in the lower part of the bore-hole. Moreover, the principal cooling effect of the currents would occur towards the bottom of the hole, where the cold water coming down from above would tend to accumulate, because there would be no warm currents coming up from below to disturb it.

When, therefore, we inquire how far the observations made with the geo-thermometer can be depended upon as having given the true temperature of the rock in mass, two questions present themselves. First, could the water enclosed between the disks assume eventually the temperature of the rock mass, if the instrument were left down long enough? And secondly, if it could do so, was it in fact left down long enough?

It appears that this result could be only partially attained, however long it were left in the bore-hole. The convection currents in the water would affect the temperature of the column at the upper and under surfaces of the enclosing disks of the instrument. And even if the disks were purposely made of such badly conducting material as to prevent any temperature effect from the currents being carried through the disks, still such effect would be conducted round the edges of the disks, through the substance of the rock itself; consequently, however long the instrument might have been allowed to remain down, the temperature of the water enclosed between its disks would have been to some extent affected by the convection currents; and their tendency in the lower parts

of the column would be to reduce the temperature, and diminish the rate of increase.

A method of obviating this source of error is mentioned in the British Association Report, already referred to, as having been lately suggested by Sir Wm. Thomson. It consists in using a series of india-rubber disks placed at considerable distances apart.

Putting the above-discussed source of error aside, we come to our second inquiry, whether the geo-thermometer was in the present case left down long enough for the water between its disks to assume the temperature of the rock mass[1]? Let us, then, consider the conditions of the system before the geo-thermometer is introduced. We have a very long vertical column of water enclosed in a cylindrical hole within a mass of rock, the rock extending to an infinite distance both sideways and downwards. The water in the bore-hole at any given depth has, in consequence of the currents, a temperature which differs from that of the rock on the same horizon at a distance from the hole. This temperature of the water may be higher or lower than that of the rock; but we will suppose it lower, as it will be in the deeper parts of the hole. The rock surface of the bore-hole being constantly laved by the water, has been brought to the same temperature as the water. It necessarily follows from this that if we could examine the temperatures of the rock at greater and greater distances laterally from the bore-hole, we should find them become higher and higher, and we should have to penetrate to some considerable horizontal distance before the increase came to an end, and when it did so we should feel assured that at last we had reached rock of the true temperature for the depth. The greater might be the difference between the temperatures of the water and the rock in mass, the further

[1] It appears that at the depth of 1100 feet an observation of ten hours' duration gave the same result as one of nineteen hours; whence it was concluded that ten was long enough. But considering the great thermal capacity of water, and the small changes which, except just at first, might be expected in the rock, as well as the difficulty of the operation, this trial can hardly be held conclusive against the probability of a further increase of temperature, if the geo-thermometer had remained down longer.

we should have to penetrate for that purpose, and this would be furthest in the lower portions. Now suppose the currents interrupted by lowering the geo-thermometer, so that the heat ceases to be conveyed away from the rock surface of the bore-hole; and we will now dismiss the consideration of the effect of the water above and below the disks of the instrument. The heat which flows out of the sides of the bore-hole into the imprisoned water is now no longer conveyed away by the currents, and begins to accumulate there. The rock in the neighbourhood of this water begins to get warmer. The region of the true rock-temperature approaches nearer and nearer to the bore-hole, until at last it reaches it, and then, and not until then, the imprisoned water assumes the true temperature of the rock. This process will require time, and that a long time. In the first place, on account of the great capacity of water for heat, it requires much more heat to warm up a volume of water through a given range of temperature than it would require to warm up an equal volume of rock. In the second place, it requires a long time for the heat to travel through the rock to reach the water. And although the conductivity of rock salt is said to be considerably higher than that of ordinary rock, so that the heat would pass more quickly through it[1], yet there will be a compensating effect in the present instance, because, on account of the greater conductivity, the true rock temperature would not be reached within so small a distance from the bore-hole, so that the heat would have a longer distance to travel before the true temperature could be restored. That the geo-thermometer was not left in place sufficiently long for this purpose seems to be proved by the following instance of the extreme slowness with which the passage of heat under such circumstances takes place. From comparing the British Association Reports for 1872 and 1873 we gather that, in the course of the observations made at the artesian well of La Chapelle, at St Denis, it was noticed that

[1] Whether this was the case with the rock salt *in situ* here is doubtful, for if it were so, the rate of increase ought, perhaps, to have been less than the normal rate. Such was not the case. It is however possible that the flow of heat was greater, see p. 6.

the temperature at the bottom of the hole was much affected by the action of the "trepan" or boring tool. It appears to have been expected that this effect would have passed off in a few days. Accordingly, a week after the tool was stopped observations were taken, and there was found what was thought to be an unaccountably sudden increase of nearly 8° F., in 60 feet of descent, near the bottom of the hole. But in the following year a second set of observations were made "some months" after the boring had been suspended, and the abnormal increase was found to have entirely disappeared. We learn, then, that *a week* was not sufficient for the rock wall of the bore-hole to regain its balance of temperature after having been disturbed through about 7° F. How much longer was required we have not the means of knowing. It seems a necessary inference that a few hours would be quite insufficient for a correct observation, and the time allowed at Sperenberg did not exceed such an interval. The conclusion must therefore follow that, on both the accounts referred to, the geo-thermometer must have shown in the lower depths a temperature less than the true temperature of the rock of the earth's crust existing at that depth.

The reasons given above seem quite sufficient to account for the diminished rate of increase of temperature indicated in the table; and they would lead to the conclusion that in all observations made in bore-holes of a sufficient width to allow convection currents full play, the temperatures taken at the lower parts will—even with the geo-thermometer—be less than the true rock temperature. The conclusion appears a legitimate one that the diminution in the rate of increase in a bore-hole, even when the convection currents are temporarily shut off, does not necessarily imply that there is any such diminution of the rate in the body of the rock; or, in other words, that the temperature curve may be a straight line within the rock, irrespective of differences in the conductivity of the strata, although that given by the geo-thermometer be concave to the axis of depths. It will also follow that the mean rate of increase for the whole depth within the rock will exceed that shown by the geo-thermometer. The mean increase, for instance, in the above table, between the temperature of the surface and that at

4042 feet, is 1° R. for 129 feet, or 1° F. for 57 feet; and it is stated in the Association Report that, when the observations are corrected for pressure, the mean rate to 3390 feet is 1° F. for 51·5 English feet. This is about the usual average. We may therefore conclude that at Sperenberg the actual rate of increase in the body of the rock is greater than this.

The most important conclusion from the above is that, in spite of the decreasing rate shown at Sperenberg (and also at St Louis, and perhaps other places), the old assumption is probably correct, that for such depths as have been reached, the rate—if it could be truly observed, and setting aside local causes of disturbance—is probably a uniform rate, amounting to, or, as the above reasoning would lead us to infer, rather exceeding, on the average, 1° F. for 51 feet of descent.

In the Proceedings of the Royal Society for 1886, Professor Prestwich gave a general list of all the observations of which he had been able to find a record. These embraced 530 separate observations at points in mines and depths in wells ("stations"), distributed over 248 localities. In his exhaustive discussion of these he was able to select only seven coal-mines, eight metallic mines, and eight wells, in which he considered that all the precautions needful for successful observation had been used and recorded. The result at which he arrived was that,

for coal mines, the maximum rate is	1°	for	45	feet of descent;
the minimum	1°	...	54	..................;
for metallic mines, the maximum	1°	...	38	..................;
the minimum	1°	...	49	..................;
for wells, the maximum	1°	...	44	..................;
the minimum	1°	...	53	..................:

and he considers the general means to be,

for coal-mines 1° in 49·5 feet;
for mines other than coal 1° in 43·2 feet;
for artesian wells 1° in 50·0 feet.

These means are averages, obtained by dividing the difference between the temperature at the lowest point reached, and that at the level where the seasonal variations cease to be felt, by the distance between them; which in most instances was

near a thousand, and in some more than twice, and even three times that number of feet. The final result thus estimated with extreme care shows a considerably higher value than has been usually assigned, which has been about 1° F. for 60 feet of descent. Professor Prestwich puts the general mean of the whole (not including tunnels) at 1° F. for 47·5 feet of descent. This comes nearer to Sir William Thomson's estimate of 1° for 51 feet, although it is even higher than that.

Although this is all that we know experimentally respecting the *rate* of increase of temperature within the earth, we have reason to believe that at still greater depths the temperature continues to increase, because many thermal springs deliver water at the surface at a much higher temperature than has been reached by any artificial perforations. And yet the waters yielded by artesian wells are in their degree thermal springs, so that the hotter natural springs, such at least as are not obviously situated in volcanic regions, are probably exactly in the same category with the deeper artesian wells. For instance, it appears extremely probable that the Bath waters are simply springs supplied by the rainfall on the Mendip hills. The water following the subterranean course of the limestone is kept down by the impervious beds of the basin-shaped coal-measures, and reascending to the surface along the fault from which the springs are known to rise, brings with it the temperature of the rocks through which it has percolated. The temperature is 120° F., which is 70° above what may be taken as the mean surface temperature, and would require, at the rate of 1° for 50 feet, a depth of 3500 feet to give the observed increase. This is by no means an unlikely depth to be attained by the synclinal trough of the carboniferous limestone in Somerset.

In like manner the water, which in 1880 flooded the lower levels of the Gold Hill mines on the Comstock Lode in Western N. America, had a temperature of 170° F., and would cook food. In fact, the abnormally high temperature of these mines was supposed to be due to those waters. The hot springs rose from the west wall of the lode, and were believed to arrive by the tapping of water-ways leading from the crest of the main range

of the Sierra Nevada; for they were more abundant than could be supplied by the local rainfall in so arid a district. It was thought that they rose from a great depth. But a depth of two or three thousand feet, in excess of that from which the Bath waters probably rise, would be sufficient to account for their high temperature. Some of the Steamboat Springs in the same neighbourhood are boiling hot[1].

But the phenomena of volcanos, which are situated in all regions of the globe, and of which relics remain almost everywhere among ancient rocks, point to an intensely high temperature at still greater depths. They afford, however, no clue as to what the depth may be from which their liquid ejectamenta are derived without first assuming a law of temperature.

Thus it is a most important point to be borne in mind, that we *know nothing by observation* respecting the law of increase of temperature within the earth, beyond the facts that *near the surface* the average rate of increase is 1° F. for about every 50 feet of descent, and that the temperature at greater, but unknown, depths is very high, and sufficient to fuse what, from that very cause, we term igneous rocks. This temperature cannot be lower than between 2000° and 3000° F., and may be much higher.

But if an average rate of increase of 1° F. for every 50 feet were to continue to all depths, the temperature in the interior would become enormous. In fact, at the depth of from 200 to 400 miles it would amount to from 20,000° to 40,000° F., which would be near the temperature, which, on apparently good grounds, has been attributed to the sun's surface[2]. This alone affords a strong presumption that the temperature rate cannot continue uniform to very great depths, seeing that, if it did so, by descending not much beyond a tenth of the earth's radius, we should arrive at temperatures exceeding the solar temperature.

1 "Reports of the Geol. Survey, U. S. A. 1880–1," p. 310. (Becker's Report.)

2 See Abstract of a Lecture at Roy. Institution by Sir Wm. Siemens. "Nature," vol. XXVIII. p. 19, 1883.

CHAPTER II.

INTERNAL DENSITIES AND PRESSURES.

The earth probably once wholly melted—Consists of concentric shells—Determination of mean and surface densities—Its form and dimensions—Mathematical theory of the "Figure of the Earth"—Laplace's law of density—Darwin's suggestion of other laws—Precession a test of law of density—Clairaut's theorem—Internal pressures calculated from Laplace's law—Pressure at the centre—Pressures deduced from Darwin's law—Internal densities probably owing to nature of materials—Waltershausen's theory—His arrangement of densities—Numerical values of densities according to Laplace's and Darwin's laws—The former the more probable expression of the facts.

VARIOUS suppositions may be made respecting the condition of the interior of the earth. What we actually know about it is very little. Whether any portion of it, or how much of it, is at the present time liquid can only be determined by secondary considerations. But we may feel almost certain that it was once wholly melted, and that it consists at present of concentric spheroidal shells, each of equal density throughout, and of definite form; such form having been determined by the velocity of rotation and distribution of density, coupled with the law of mutual attraction of all the mass. The ellipticity of these shells decreases from the surface towards the centre. The mean density of the surface has been estimated at from 2·56 to 2·75. The mean density of the whole is nearly twice as great. The summary of the experiments from which these constants have been determined is as follows:

1. Maskelyne, 1775, by means of the deflection of the plumbline by the mountain Schehallion determined the mean density of the earth at 5·0.

2. Cavendish, 1798, by the torsion rod and two leaden balls as attracting masses, weighing together 700 pounds, in 23 experiments determined the mean density at 5·448.

3. Reich, 1837, using one large leaden ball of 99 pounds for the attracting mass, in 57 experiments determined the mean density at 5·44.

4. Baily, 1842, using two large leaden balls, each weighing 380½ pounds, in 2004 experiments determined the mean density at 5·67.

5. Airy, 1856, by experiments with the pendulum in Harton Colliery, at a depth of 1260 feet, determined the mean density of the earth at 6·566. The mean density of the ordinary beds of rock above was determined by W. H. Miller at 2·56, and of the coaly beds at 1·43. Airy considered that his determination might "compete with the others on at least equal terms."

6. Col. James, 1856, by observations on the deflection of the plumbline at Arthur's Seat determined the mean density of the earth at 5·316. The mean density of the rocks at Arthur's Seat was determined at 2·75.

7. Mallet estimates the weight of a cubic foot of granite at 178·34 pounds, which makes its density 2·69.

8. Waltershausen finds the specific gravity (or density) of

Albite..	2·52
Quartz	2·62
Limestone	2·63
Mica	2·92
Mean	2·66

which he takes as the mean density of surface rock. Such a mean, however, would require these minerals to occur in equal proportions in the rock.

We have then for the mean densities according to the experiments respectively of

	Whole earth.	Surface rock.
Maskelyne............	5·0	
Cavendish	5·448	
Reich..................	5·44	
Baily	5·67	
Airy and Miller ...	6·566	2·56
James	5·316	2·75
Mallet		2·69
Waltershausen		2·66

If we select Col. James's determination for the density of surface rock, viz. 2·75, and double it, we get 5·50, which is a fair representation of the mean value of the density of the whole earth; being somewhat greater than the mean 5·375 if Airy's determination is omitted, and somewhat less than the mean 5·573 if it be included.

These are the values used by Archdeacon Pratt in his "Figure of the Earth."

Although roughly speaking the earth may be called spherical, the measurement of arcs of the meridian in various parts of the world has resulted in the discovery, that the geometrical figure most accurately representing it is an oblate spheroid of revolution, whose equatorial axis is

$$a = 20926202 \text{ feet},$$

and the polar axis is

$$c = 20854895 \text{ feet}[1].$$

Their difference is therefore

$$a - c = 71307 \text{ feet}.$$

The ratio $\dfrac{2(a-c)}{a+c}$ is called the ellipticity, and its value may be taken as $\frac{1}{293}$.

If the assumption be made that the earth has acquired its present form from being a mass of liquid, acted on by the mutual gravitation of its parts and by the centrifugal force of rotation, it will follow that it consists of strata, of which those nearer to the centre are more dense than those more remote. The interior surfaces over which the pressures are equal will also be of equal densities ("equipotential" or "level" surfaces).

[1] Clarke's "Geodesy," p. 319. Oxford, 1880.

If it be now assumed that these surfaces are spheroidal and of small ellipticity, the mathematical expression of these hypotheses leads to two differential equations of the second order, which require, as a preliminary step to their solution, the selection of a law connecting the density with the pressure at each of the surfaces; and this law must be empirical, on account of our entire ignorance of the facts.

Now it is not probable that the increase of density towards the centre should be caused solely by the increased pressure. It is more probable that also heavier materials should be found at greater depths. But necessity obliges the supposition to be made, that the increase of density follows (not necessarily is caused by) the increase of pressure according to some law expressible by mathematical symbols. If a function expressing this relation can be decided on, it may then be introduced into one of the differential equations, which being supposed solved, the needful arbitrary constants might be determined, and the pressure, density, and ellipticity, of every shell, could be found: proving that the conditions of equilibrium would be satisfied by the presupposed arrangement of the revolving mass in spheroidal shells; the exterior stratum, where the ellipticity and density are known by observation, being of course one of them[1].

But since there are three quantities, the pressure, density, and ellipticity, of a shell, to be determined, and only two equations, an assumption giving a third equation must, as already remarked, be made, to connect the density with the pressure. Consequently the form of this function must be arbitrary. The equation not hitherto employed then enables constants to be assigned to the function, so that it may fulfil two conditions. These may be, either that the mean density and the density of the surface shall agree with those of the earth, or that the mean density and the ellipticity at the surface shall do so.

Starting then with the necessary proviso, that the mean density agrees with that of the earth, either the density of the surface, or the ellipticity of the surface may be chosen as the second arbitrary constant, to be equated to the known value,

[1] See Clarke's "Geodesy," p. 81.

and then the outcome of the remaining one (in the first case the ellipticity of the surface, and in the second the density of the surface) being compared with the known value, will afford a test of the admissibility of the law of density which has been arbitrarily assumed.

The law, assumed by Laplace to express the relation between the density and the pressure, is that which has been usually adopted. His hypothesis is, that the increase of the square of the density is proportional to the increase of the pressure[1]. The two constants being determined from the mean density, viz. 5·5, and the surface density, 2·75, this law leads to a surface ellipticity of $\frac{1}{293}$; which happens to be just the observed ellipticity: and this circumstance affords a strong argument, so far, in favour of the supposed law of relation.

Professor Darwin in 1883 suggested other relations between the density and pressure; and from his investigation we gather that, when the increase of the logarithm of the density is assumed to be proportional to the increase of the pressure[2], the two constants being determined from the mean density and the surface ellipticity, this assumption leads to a surface density of 3·7, which is greater than that of the average of rocks of the earth's crust[3]. Nevertheless it accords very well with the density of the more basic rocks, which occur in many places at the surface, and which there is reason to think very generally lie at no great depth. And he remarks, "in any theory of the earth's density, a sudden change in the thin shell on the surface could not be taken into account, and the numerical value for the surface density should be taken from below the intumescent layer if it exists[4]."

The astronomical phenomenon of the Precession of the

[1] If we consider the stratum where the pressure is p and the density ρ, Laplace's law may be expressed $\frac{d(\rho^2)}{dp} = \text{a constant}$: whence $\frac{d\rho}{dp} \propto \frac{1}{\rho}$, or the increase of density for a corresponding increase of pressure is inversely proportional to the existing density at the depth in question.

[2] *i.e.* $\frac{d(\log \rho)}{dp} = \text{constant}$.

[3] "Proceedings Roy. Soc." No. 229, p. 8, 1883.

[4] Ibid.

Equinoxes affords a further test of the admissibility of any assumed law of density, because the amount of precession involves the moments of inertia of the earth about its equatorial and polar diameters; and will therefore depend upon the arrangement of the matter about those diameters; in other words, upon the law of density of the interior. When therefore an arbitrary law has been selected, the possibility of its fairly representing the true state of the case may be further judged of, by inquiring whether it would give the correct amount of precession: and if it does that, as well as satisfy the geodetic test, it greatly increases the probability that it has been properly chosen: and being at the same time based on the fluid theory, that that theory is true. Laplace's law of density satisfies the test very accurately, and so likewise does that other law lately proposed by Prof. Darwin. But the fact that two different arrangements of matter satisfy the tests equally well, weakens the case for each, since they cannot be both right, although it shows that either is tenable to the exclusion of the other.

"Clairaut's theorem" is a mathematical law which expresses the variation of gravity in different latitudes, in as far as it depends upon the ellipticity of the surface of an attracting spheroid, and the centrifugal force at its equator. Professor Stokes has shown that, provided the surface of the earth is spheroidal and of small ellipticity, and is an equipotential surface (which it is) under the influence of the attraction of the whole mass and the centrifugal force, then, whatever be the internal arrangement of densities consonant with this condition of the outer surface, Clairaut's law will hold good. Now it is evident that any condition suitable to produce equipotential surfaces, of which that of the ocean will be one, will find a place among those arrangements; and therefore for such Clairaut's law will hold good. But since other arrangements besides these will also satisfy this law, the fact that the variation of gravity in different latitudes does follow Clairaut's law, is no *proof* that the internal strata are arranged in equipotential surfaces, and that consequently the earth is, or was once fluid: although it makes it probable.

It seems then on the whole that the remarkably refined mathematical investigations, which have exercised the ingenuity of the greatest masters of the science from Newton to the present day, do not definitely inform us beyond what was stated at the beginning of this chapter; namely, that the earth was almost certainly once wholly melted, and that it consists at present of concentric spheroidal shells, each of equal density throughout, and of definite form; such form having been determined by the velocity of rotation and law of density, coupled with the law of mutual attraction of all the mass. The ellipticity of these shells decreases from the surface towards the centre.

The accuracy with which the two laws of density mentioned satisfy both the geodetic and astronomical tests causes it to be an interesting inquiry what the internal pressures would be at different depths on those hypotheses.

Let p be the pressure at the distance r from the centre of the sphere, ρ' the density and g' gravity at that level. Then as r is increased p is diminished and we shall have

$$\frac{dp}{dr} = -g'\rho' \qquad (1).$$

Since the attraction of the portion of the sphere exterior to the shell, whose radius is r, upon a particle within it is nothing, the value of g', due to the remaining portion, which may be supposed collected at the centre, will be,

$$g' = \mu \frac{\int_0^r 4\pi r^2 dr \rho'}{r^2};$$

μ being a constant depending upon the unit of force.

Now Laplace's equation, which leads to the law of density assumed by him, is

$$\rho' = Q \frac{\sin qr}{r},$$

where Q and q are constants to be determined. Consequently if P be the central density it will be given when $r = 0$;

[1] Pratt's "Figure of the Earth," 4th Edition, p. 112, 1871.

or $$\mathrm{P} = Q\frac{\sin qr}{qr}q, \text{ when } r = 0,$$

$$= Qq, \text{ is the density at the centre.}$$

Also the surface density will be given by putting

$$r = a;$$

$$\therefore \rho = Q\frac{\sin qa}{a}.$$

Hence, substituting for ρ' its value $Q\dfrac{\sin qr}{r}$ and integrating, we have,

$$g' = \mu\frac{4\pi Q}{q^2r^2}(\sin qr - qr\cos qr),$$

and therefore,

$$g'\rho' = \mu\frac{4\pi Q^2}{q^2r^3}(\sin^2 qr - qr\sin qr\cos qr).$$

Hence, g and ρ being the values of g' and ρ' at the surface,

$$\frac{g'\rho'}{g\rho} = \frac{a^3\sin^2 qr - qr\sin qr\cos qr}{r^3\sin^2 qa - qa\sin qa\cos qa},$$

$$= A\frac{\sin^2 qr - qr\sin qr\cos qr}{r^3}.$$

Substituting for $g'\rho'$ from (1), and integrating by parts,

$$\frac{p}{g\rho} = \frac{A}{2}\frac{\sin^2 qr}{r^2} + C,$$

when $$r = a, \ p = 0;$$

$$\therefore p = \frac{g\rho A}{2}\left(\frac{\sin^2 qr}{r^2} - \frac{\sin^2 qa}{a^2}\right).$$

If we write na for r, and restore the value of A, this may be put in the form

$$p = \frac{g\rho a}{\sin 2qa(\tan qa - qa)} \times \left(\frac{\sin^2 qna}{q^2n^2a^2}q^2a^2 - \sin^2 qa\right).$$

This gives the pressure at any depth, the only variable quantity in the expression being

$$\frac{\sin^2 qna}{q^2n^2a^2}.$$

which at the centre, where $n = 0$, becomes unity. Hence the pressure at the centre is

$$\frac{g\rho a}{\sin 2qa\,(\tan qa - qa)}(qa + \sin qa)(qa - \sin qa).$$

When the constants Q and q are determined in accordance with the mean values above given of the density of the whole earth (5·5) and of the surface density (2·75) it is found that

$$qa = 2·4605 = 140^\circ\, 58'\, 35'' ;$$

and $$Qq = 10·74,$$

which is therefore the density at the centre of the earth upon the hypothesis of Laplace's law being true.

If we define the mean sphere as one of equal volume with the spheroid, then its radius $= \sqrt[3]{a^2c}$

$$= 20902404 \text{ feet}$$

$$= 3958·78 \text{ miles},$$

which we will take to be the value of the mean radius.

Then, with the value of qa above given we obtain for the pressure upon a square foot at the earth's centre

6,351,810,000 pounds.

Reducing this to atmospheres at 14·7 pounds per square inch, the fluid pressure at the centre of the earth will be found to be

3,000,660 atmospheres.

We will now turn to Prof. Darwin's law of density, which gives equally good results with Laplace's so far as has been mentioned. The general form of the law which he has investigated may be written,

$$\rho' = Cr^{-x}.$$

But in the particular case referred to x is to be unity, or

$$\rho' = \frac{C}{r}.$$

At the surface this becomes,

$$\rho = \frac{C}{a},$$

whence $$\rho' = \rho \frac{a}{r};$$

or the density varies inversely as the distance from the centre.

As in the former instance we have

$$\frac{dp}{dr} = -g'\rho' = -g'\rho \frac{a}{r}:$$

and $$g' = \mu \frac{\int_0^r 4\pi\rho' r^2 dr}{r^2},$$

$$= \mu \frac{\int_0^r 4\pi\rho a r dr}{r^2},$$

$$= \mu\, 2\pi\rho a;$$

which shows that, with this law, gravity is the same at all depths.

Hence $$\frac{g'\rho'}{g\rho} = \frac{\rho'}{\rho} = \frac{a}{r}.$$

And $$p = -\int g'\rho' dr,$$

$$= -g\rho \int \frac{a}{r} dr,$$

$$= -g\rho a \log r + C.$$

At the surface $p = 0$, and $r = a$,

$$\therefore p = g\rho a \log \frac{a}{r},$$

where it will be observed $g\rho a$ is the weight of a column of surface rock of the length of the radius.

At the centre, where $r = 0$, the pressure becomes infinite.

Since $$\frac{a}{r} = \frac{\rho'}{\rho},$$

and that consequently $p = g\rho a \log \dfrac{\rho'}{\rho}$,

it follows that

$$d(\log \rho') \propto dp;$$

which gives the law of relation between the density and pressure

in the form already mentioned, in which it may be readily compared with Laplace's law, which is

$$d(\rho'^2) \propto dp.$$

Prof. Darwin says, "the infinite density and infinite pressure, which occur in this solution actually at the centre, may be avoided by imagining the centre occupied by a rigid homogeneous nucleus of very small radius." To find what the radius (r) of such a nucleus of density P must be, to produce the same attraction at the surface as when the same space is occupied by matter whose density follows the law, we observe that the attraction of the latter at the surface is

$$\mu \frac{2\pi\rho r^2}{a},$$

ρ being Darwin's surface density, viz. 3·7; while the attraction of the central block of density P, also at the surface, will be

$$\mu \frac{4}{3} \frac{\pi \mathrm{P} r^3}{a^2}.$$

Equating these, and putting for P the central density according to Laplace's law, viz. 10·74, we get,

$$r = 0{\cdot}51a = 2017 \text{ miles}.$$

This is too large a value not to alter sensibly the precessional constant, and would therefore be inadmissible. But the central block may be composed of substances of higher density still. Yet even with the density of platinum (22) its radius would need to be a thousand miles. This would seem to point to Laplace's law being of the two the nearer representation of the truth.

The increase of density in approaching the centre will depend partly upon the pressure, and partly upon the nature of the materials. We do not know enough about the ultimate constitution of matter to reason satisfactorily about the effect of pressure. It has been thought that by that means the density of a substance can be increased up to a certain limit, but no further[1]. If this be so, it renders it probable that the high density of the central parts of the globe is due to the intrinsic

[1] "That substances are composed of a system of points, which are mere centres of force attracting or repelling each other," is shown to be an untenable theory by Maxwell, "Heat," 5th Ed., 1877, p. 86.

nature of the materials there, and that a large proportion of the heavy metals occupies that position. Waltershausen in his "Rocks of Sicily and Iceland" has formed a theory of the earth on this basis. It is thus epitomised by Mr Clarence King in the "Geological exploration of the fortieth parallel," vol. I. p. 710[1], "To sum up the theory of Waltershausen; the earth is a hot globe, of which a considerable portion is fluid, an unknown fraction of the centre having been rendered solid by the raising of its fusion temperature by pressure. The downward increment of density is expressed by the chemical increment of the heavy bases: and the fluid region directly under the crust consists first of a feldspathic and acidic magma, which passes downwards by successive replacement of bases into an augitic, and finally into a magnetitic magma."

The formula used by Waltershausen[2] for the calculation of the density ρ' at a distance r from the centre, expressed as a fraction of the radius, is of the form $\rho' = \mathrm{P} - (\mathrm{P} - \rho)\, r^2$: and, although this law cannot be considered of authority, the densities arranged by him according to it are not without interest.

DENSITIES ACCORDING TO WALTERSHAUSEN.

RADIUS.	DENSITY.	
1·00	2·66	
0·99	2·79	
0·98	2·93	
0·97	3·07	Lime.
0·96	3·20	Magnesia.
0·95	3·34	
0·94	3·47	
0·93	3·60	
0·92	3·72	
0·91	3·85	
0·90	3·99	Alumina.
0·80	5·15	Iodine, Iron oxide.
0·70	6·29	Tellurium, Chromium.
0·60	7·09	Zinc, Iron, Tin.
0·50	7·85	Cobalt, Steel.
0·40	8·47	Uranium, Nickel.
0·30	8·96	Copper.
0·20	9·31	
0·10	9·51	
0·00	9·59	Bismuth, Silver.

[1] Washington, Government printing office, 1878.

[2] "Rocks of Sicily and Iceland," p. 315.

The density, which he has placed at the centre, is not so high as that required by Laplace's law, viz. 10·74. There are however substances of still higher densities than that; for instance,

Lead ....11·0,
Mercury ....13·0,
Gold ....19·0,
Platinum............22·0.

These metals may possibly occur in appreciable quantities in the central regions.

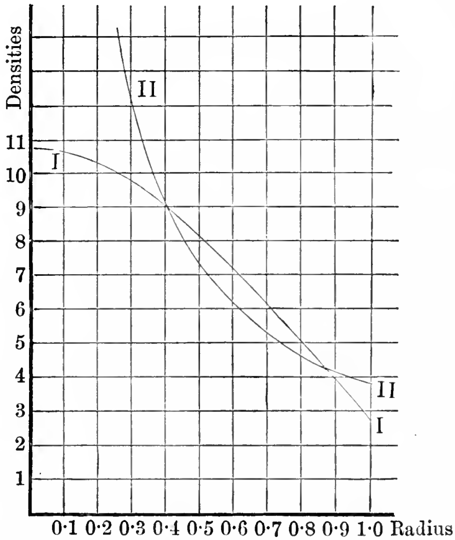
The densities according to Laplace's and Prof. Darwin's laws have greater authority, because both of them equally well satisfy the geodetic and astronomical tests. They are given below for intervals of one tenth of the radius.

DENSITIES CALCULATED FROM THE LAWS,
I. OF LAPLACE, II. OF DARWIN.

RADIUS.	DENSITY.	
	I.	II.
1·00	2·75	3·7
0·9	3·88	4·1
0·8	5·03	4·6
0·7	6·17	5·3
0·6	7·25	6·1
0·5	8·23	7·4
0·4	9·09	9·2
0·3	9·80	12·3
0·2	10·32	18·5
0·1	10·64	37·0
0·0	10·74	∞

The curves given in the diagram will make the characters of these two laws apparent, and they show that, between the surface and 0·4 of the radius, the densities range moderately near each other; while, between 0·4 of the radius and the centre, the rate of increase of Laplace's densities gradually diminishes, and Prof. Darwin's rapidly increases up to infinity. If the densities of the substances in the central parts of the earth are due rather to their intrinsic nature than to condensation by pressure, it is clear that Laplace's law is the more probable representation of the reality: for it must be remembered that, although based

upon a supposed relation between density and pressure, it does not necessarily imply that the density is the result of the pressure.



CHAPTER III.

CONDITION OF THE INTERIOR.

The question proposed whether a fluid condition of the interior partially exists—Connection between this question and that of the law of internal temperature—Two modes of attacking the question—The first based upon the phenomena of Precession and of the tides—Hopkins' argument for rigidity from Precession—Abandoned by Sir W. Thomson—And by Prof. Darwin—The argument for rigidity from the tides—The fortnightly tide—Indian observations—Darwin's conclusions from them on the "equilibrium theory"—Admits that theory to be incorrect—The "dynamical tide"—Comparison between it and the observed tide inconclusive.

To guide us in our reasoning concerning the condition of the interior we have the following data:

The observed rate of increase of temperature near the surface is perfectly compatible with the existence of such a temperature within the earth as would fuse all known substances, and fused substances continue to flow up from below, and, what is more, there is almost certain evidence that the earth was at one time wholly fluid. The question arises, does this fluidity still normally exist within it, wholly or partially?

The true law of increase of temperature is inextricably mixed up with this other question of the condition as to solidity or fluidity of the interior; because *any* law of increase of temperature is compatible with the circumstance that the increase is proportional to the depth near the surface. For if we represent the law of temperature by a curve starting from

the surface whose axis of abscissæ is vertical, this merely amounts to saying that any number of different curves may be drawn which have the same tangent at the origin. But the law of increase of temperature below that outer part or crust of the earth, which we know to be solid, will be subject to the laws of convection if the interior be fluid, modified by change in density and in the value of gravity, and by those of conduction if it be solid. Hence if there be a change of state from fluid to solid, there will necessarily be a break in the law at the depth where that occurs; and it will then be further complicated by important thermal changes which may accompany the change of state.

Now the question as to whether the interior of the earth is at the present time solid or fluid, or partly solid and partly fluid, apart from geological considerations may be attacked in two ways. The first of these is by inquiring what would be the difference of effect that bodies exterior to it, namely the sun and moon, would have upon the motions of the earth in either case; and secondly, by considering the sequence of events according to which a molten globe, such as the earth once was, may have passed into its present state.

The argument for rigidity derived from the precession of the equinoxes used to be, that, in explaining this astronomical phenomenon upon dynamical principles by the attraction of the sun and moon upon the protuberant matter of the spheroid at its equator, the moment of inertia of the earth, which measures the opposition that it offers to a change of angular velocity, had always been taken account of in the calculations, as if the earth was rigid; and the result was found to agree with the observed amount of precession. This agreement was supposed to prove, that the assumption of rigidity was necessarily true to nature.

Mr Hopkins, in the "Phil. Trans." for 1839-40-42, investigated the consequences which, according to certain assumptions, would have resulted from the effect of internal fluidity upon this phenomenon of precession of the equinoxes, and he came to the conclusion that the solid crust of the earth could not be less than from 800 to 1000 miles thick.

His argument was assailed by Delaunay[1], but strongly supported by Sir W. Thomson and Archdeacon Pratt: while General Barnard[2] believed that Hopkins' result was vitiated by an oversight. It is however now scarcely worth while to notice this argument further, because Sir W. Thomson has himself given up this particular reason against the doctrine of a fluid interior. "Interesting in a dynamical point of view as Hopkins' problem is, it cannot afford a decisive argument against the earth's interior liquidity[3]." To this change of opinion he was led by a conversation with Prof. Newcomb in America. Moreover the objection has been finally disposed of by Prof. Darwin, who has found that "The precession of a fluid spheroid is the same as that of a rigid one, which has an ellipticity equal to that due to the rotation of the spheroid[4]." Here we have a remarkable instance of the final abandonment of an argument, which, from the portentous difficulties of comprehending it, had proved too hard for geologists to assail.

But there is another manner in which the sun and moon affect terrestrial motion; namely by raising ocean tides. Meanwhile the same attractive force is exerted by their masses upon the body of the earth itself as upon the water which partly covers it; and it has been thought that a test ought to be thereby afforded of the condition of the interior whether it be liquid or solid, and if solid to what degree rigid. For it has been argued that if the earth is not exceedingly rigid the surface of the globe itself would be raised in tides as well as the water which covers it, and since the measurable tide is the distance between the floor of the ocean and the surface of the sea, it is plain that if the floor of the ocean was drawn up simultaneously with the surface of the sea, the tide would be thereby diminished. Sir W. Thomson has said: "The solid crust would yield so freely to the deforming influence of the sun and moon, that it would simply carry the waters of the ocean up and down

[1] "Geol. Mag." vol. v. p. 507.

[2] "Problems of Rotary Motion." "Smithsonian Contributions," No. 240. New Addendum, p. 42. vol. xix. 1871.

[3] Sectional Address to British Association, 1876.

[4] "Phil. Trans." Part II. 1879, p. 464.

with it, and there would be no sensible tidal rise and fall of water relatively to land[1]."

It must not however be lost sight of that this deformation of the body of the earth has been estimated upon the supposition that the materials of which it is composed are incompressible as regards volume.

The subject is one of extreme complexity. It is necessary to calculate what the height of a particular ocean tide would be theoretically, and to compare it with the actual height of the same tide, and so, by noting the discrepancy between them, to form an estimate of the amount of yielding of the globe which would account for such discrepancy.

The semidiurnal tide is so complicated by the irregular configuration of the ocean basins and by other causes that it is unsuitable for this comparison; and the fortnightly declinational tide is preferred. This tide consists in a slight heaping up of the ocean about the equator and a lowering at the poles once a fortnight when the moon is crossing the earth's equator, alternating with a lowering about the equator and heaping up at the poles when she attains her extreme north and south declination.

As respects the actual height of the ocean tides, great advances in knowledge have been made through the adoption of systematic observations at a great number of stations by the Indian Government[2]. These were taken at all the principal ports of India, and at such points on the coast lines as were best suited for investigation of the laws of the tides. During five years observations were made at several points, and treated by the method of harmonic analysis. The constants for amplitude and epoch are determined for every tidal component both of long and short periods, and with their aid tide tables are now prepared and published annually for each of the principal ports[3]. It was

[1] Sectional Address to British Association, 1876.

[2] Prof. Darwin "on a Numerical Estimate of the Rigidity of the Earth," "Nature," Nov. 2, 1882. Also Thomson and Tait "Nat. Phil." 2nd Ed. 1883, art. 847.

[3] See Address to Geographical Section, British Association, by Gen. J. T. Walker, C.B., F.R.S., 1885, reported in "Nature," vol. XXXII. p. 486.

with these that Prof. Darwin made his investigation of the effective rigidity of the earth. In the first place he calculated the height of the tide on the equilibrium theory, which neglects the inertia of the water, and the result at which he arrived "tended to show that the earth is solid to its core, and that the geological hypothesis of a fluid interior is untenable." He adds "Whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small and the effective rigidity is very considerable, not so great as steel as was at first surmised, but sufficient to afford an important confirmation of the justice of Sir W. Thomson's conclusion as to the great rigidity[1]."

Subsequent investigations by Prof. Darwin have however modified these results; for on calculating the fortnightly tide, taking the inertia of water into account he found that, neglecting friction on the ocean bottom, with an ocean 1200 fathoms deep, the tide is smaller than on the equilibrium theory, being $\frac{1}{7}$th of it at the pole and nearly $\frac{1}{2}$ at the equator. With a deeper ocean the tide soon becomes equal to the equilibrium tide[2]. It certainly seems to be properly admissible to neglect friction, because the action is not of the nature of ordinary friction, but of a viscous shear, which in such a liquid as water would offer no appreciable resistance to the motion of the general body of the ocean at a moderate distance from the surface of contact with the solid bottom. But whatever the effect of friction may be, it must tend to make the tide smaller.

Among the conclusions which Prof. Darwin has drawn from his latest investigations on this question we read, "It appears that, with such oceans as we have to deal with, the tide of long period is certainly less than half its equilibrium result[3];" and,

[1] See note 2 on p. 39.

[2] "On the Dynamical Theory of the Tides of long Period," Roy. Soc., Nov. 25, 1886. "Nature," Jan. 20, 1887.

[3] According to Sir W. Thomson, "The sum of the rise from lowest to highest at Teneriffe, and simultaneous fall from highest to lowest at Iceland, in the lunar fortnightly tide, would amount to 4·3 inches if the earth were perfectly rigid, or 2·9 inches if the tidal effective rigidity were only that of steel, or 1·7 inches if the tidal rigidity were only that of glass." ("Nat. Phil." 2nd Ed.

"The present result shows that it is not possible to attain any estimate of the earth's rigidity in this way; but, as the tides of long period are distinctly sensible[1], we may accept the investigation in the 'Natural Philosophy' as generally confirmatory of Thomson's view as to the effective rigidity of the earth's whole mass."

We appear however to be landed in this difficulty; that the actual tide is thought to be nearly as great as the equilibrium tide, whereas the dynamical tide, which ought more nearly to represent nature, is not half the equilibrium tide. Consequently the actual tide is nearly double the properly calculated theoretical tide; whereas, if the earth were absolutely rigid, it ought just to equal it. In short, the tidal argument for rigidity seems to prove too much.

art. 845.) These are of course estimated on the equilibrium theory. It seems to be on the discrimination of these small effects amidst the complicated phenomena of the tides that the estimate of rigidity rests.

[1] "The very long- and very short-period tides are thus beset with difficulties of a different nature: for the first, the vortex motion of the supposed internal liquid produces great tidal effective rigidity; for the second, the inertia of the external liquid covering comes into play. Both theories point to the fortnightly tide as the one which alone can settle the question. So many difficulties have occurred in the work of tide-registering that the Tidal Committee of the British Association appears to be still doubtful, whether there really is an appreciable fortnightly tide. If there is, we shall be entitled to say that the tidal effective rigidity of the earth is too great to allow us to suppose it to consist of a liquid mass covered with a thin solid crust." This conclusion however would still rest upon the hypothesis of the liquid being incompressible. (Love, "On the Free and Forced Vibrations of an Elastic Spherical Shell containing a given mass of liquid." "Proc. London Math. Soc." vol. XIX. p. 206, 1888.)

It is well to recollect that the "fortnightly tide" is a mathematical entity; that is, calculation shows that, on the hypothesis of the earth's rigidity, the moon's action upon the ocean ought to produce a small tide of that period. But Mr Love seems to doubt whether such a tide has hitherto been discriminated among the visible oscillations which affect the waters.

CHAPTER IV.

CHANGE OF DENSITY ON SOLIDIFICATION.

Hopkins on the cooling of the globe—Relative densities of substances in the molten and solid states—Sir W. Thomson's opinion—Modified by experiments of Whitley, D. Forbes, and Messrs Chance—Prof. Wadsworth—Observations at Volcanos by Mr Coan, Dr Johnston-Lavis, and Prof. Palmieri—Description of Kilauea by Commander Wilkes—By Capt. Dutton—Temperature how sustained—Changes since Dana's visit in 1840—Level of the solid surface sinks when the lava sinks—Emerson's visit after the emptying of 1886—Formation, break-up, and ingulfment of the lava crust described by Dutton—And by Mark Twain—Suggestion that the ingulfment may be caused by convection currents—Mr Green's view somewhat similar—Evidence on the whole question not decisive.

In the present chapter we shall consider the sequence of events, according to which a molten globe may have passed into its present state; which is the second mode of attacking the question whether the interior is solid or liquid.

Mr Hopkins, in his "Researches in Physical Geology[1]," has gone into this question somewhat fully. He considered that, so long as the matter of the earth retained a sufficiently high state of fluidity to admit of the circulation of convection currents, no crust could form: but that when, by those means, the temperature had been so far reduced that currents became arrested, immediately a crust would be formed. "Since the heat in-

[1] "Trans. Roy. Soc." Part II. 1839, quoted in his "Report to the British Association," 1848.

creases with the distance from the surface, while the mass is cooling by circulation, the tendency to solidification, so far as it depends on this cause, will be greatest at the surface, and least at the centre. But on the other hand, the pressure is least at the surface, and greatest at the centre; and consequently the tendency to solidify as depending on this cause will be greatest at the centre, and least at the surface[1]." For want of experimental evidence, "the only conclusion at which we can arrive is this, that if the augmentation of temperature with that of the depth be so rapid that its effect in resisting the tendency to solidify be greater than that of the increase of pressure to promote it, there will be the greatest tendency to become *imperfectly fluid,* and afterwards to solidify, in the superficial portions of the mass: whereas, if the effect of the augmentation of pressure predominate over that of temperature, this transition from perfect to imperfect fluidity, and consequent solidity, will commence at the centre[2]."

Assuming the latter to be the case, when the mass should have arrived at that stage of cooling that "a solid nucleus had been formed, surrounded by an external portion of which the fluidity would vary continuously from the solidity of the nucleus to the fluidity of the surface, where, at the instant we are speaking of, it would be just such as not to admit of circulation; ......a change would take place in the process of solidification which it is important to remark. The superficial parts of the mass must in all cases cool the most rapidly, and now (in consequence of the imperfect fluidity) being no longer able to descend, a *crust* will be formed on the surface, from which the process of solidification will proceed far more rapidly downwards, than upwards on the solid nucleus." And in a note to the Report[3], he writes thus decidedly: "Supposing the earth once to have been fluid, it must be now, or have been at some antecedent epoch, in that state in which a solid exterior rests on an imperfectly fluid and incandescent mass beneath. It is important to know that this state of the earth, assuming its original fluidity, is one through which it must necessarily have

[1] *Ibid.* [2] *Ibid.* [3] "Report," p. 48.

passed in the course of its refrigeration, whatever might be the process of its solidification."

These remarks of Mr Hopkins are entirely independent of his subsequent calculations by which he considered he had proved (and was until very lately considered by the greatest mathematicians to have proved)[1] that this crust is not at the present time a thin one, but has grown to the thickness of at least not far short of a thousand miles; even if its downward growth has not already met the upward growth of the solid central nucleus.

The reasoning by which Mr Hopkins concluded that a crust would be formed (and he clearly supposed it would be supported also), seems to be assailable only on the supposition that upon solidification a sudden considerable contraction, and consequent increase of density, would occur, which would enable the fragments to sink in a fluid, too viscous to admit of the sinking of portions cooled to the *verge* of solidification.

Hopkins does not appear to have put forward any supposition regarding differences of intrinsic density of the materials of which the interior of the globe may consist. But if, as is most probable, the layers consist of substances of different composition and density, the heavier, whether liquid or solid, would sink towards the centre; and the limits of thickness of the layers, within which convection currents caused by differences of temperature could find play, would be confined to those regions throughout which the materials, if reduced to equal temperature, would become of equal density. This limitation would greatly impede the equalisation of temperature throughout the whole, and favour the formation of a crust. If then there should exist some quantity of a material, of less density when solid than the rest when liquid, however a layer of it might behave during freezing within the limits of its own depth, yet, when it was all frozen, it would necessarily form a floating crust. A crust of the more acid silicates would thus rest upon a molten layer of the more basic, and, since the latter are the more fusible, such would almost certainly at one time have been the condition, if it be not so still.

[1] *Vid. supra*, p. 37.

If however we regard the possibility of a crust floating upon a molten layer of the *same* substance, this will depend upon whether the substance in freezing contracts or expands. If it contracts, we must accept Sir W. Thomson's conclusion, "Suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable! And what of the upheavals and subsidences? They would be strikingly analogous to those of a ship that had been rammed; one portion of crust up and another down, and then all down. I may say with almost perfect certainty that whatever may be the relative densities of rock, solid and melted, at or about the temperature of liquefaction, it is, I think, quite certain that cold solid rock is denser than hot melted rock; and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava[1]."

But in fact the behaviour of melted silicates on cooling is by no means certainly known. Indeed, after the above passage had been published, Sir W. Thomson himself announced; that "some important experiments had been carried out, at the request of Dr Henry Muirhead, by Mr Joseph Whitley of Leeds. His experiments were made on iron, copper, and brass, and on whinstone and granite; and the general result hitherto arrived at seems to be, that these substances are *less* dense in the solid than in the liquid state at the melting temperature[2]." And D. Forbes stated that glass floats on melted glass (and similarly Bessemer steel on melted steel) and that Messrs Chance found the castings made from fused Rowley Rag were of the same size precisely as the wooden pattern. It is right to state however that the sand moulds were made red-hot to allow of slow cooling and devitrification[3].

Prof. Wadsworth of Cambridge, Mass., refers to many recorded experiments, which prove that several of the metals expand in passing from the liquid to the hot solid state; and adds, that there seems but little doubt, that this is the case

[1] Sectional Address to the British Association, 1876.

[2] "Trans. Geol. Society of Glasgow," vol. VI. Pt. 1, p. 40.

[3] "Chemical News," vol. XVIII. p. 191.

also with the rocks, although further careful experiments are needed[1].

But probably the best results are to be expected from observations at volcanos, to some of which we shall now refer.

An experiment was made by Mr Coan during a great eruption of Mouna Loa. In a tunnel through a lava stream, into which were several openings, they saw a swiftly flowing "river of fire" at a depth of fifty feet. Through these openings, he says, "we cast large stones. These, instead of sinking into the viscid mass, were borne along instantly out of sight[2]."

On the other hand Dr Johnston-Lavis tried a similar experiment at Vesuvius with a stream of bright orange coloured lava, with a liquidity almost of water, and he found cold lava to be of higher specific gravity than molten rock; "reversing the results of Palmieri and others[3]," and also of Mr Coan.

The following notices of observations at the crater pit of Kilauea in the Hawaiian Islands contain incidental mention of various volcanic phenomena, which are full of interest. This crater is situated on the flank of the gigantic dome of the mountain about twenty miles from the summit crater of Mouna Loa and 10,000 feet lower down. The first impression made by the sight of it is thus described by Commander Wilkes. "So striking was the mountain [Loa] that I was surprised and disappointed, when called upon by my friend Dr Judd to look at the volcano [Kilauea]: for I saw nothing before me but a huge pit, black, ill-looking, and totally different from what I had anticipated. There were no jets of fire, no eruptions of heated stones, no cones, nothing but a depression, that, in the midst of the vast plain by which it is surrounded, appeared small and insignificant." At the further end was what appeared a small cherry-red spot, whence vapour was issuing and condensing above it into a cloud of silvery brightness. This cloud however, was more glorious than any I had ever beheld, and the

[1] "On the Evidence that the Earth is Solid." American Naturalist, June, July, August, 1884. He considers solidity not proven.

[2] Dana, Exploring Expedition U.S.A., p. 210, note.

[3] "Late changes in the Vesuvian Cone, Nov. 1881." "Nature," vol. xxv. p. 295.

sight of it alone would have repaid for the trouble of coming thus far....The rushing of the wind past us was as if it were drawn inwards to support the combustion of some mighty conflagration. When the edge is reached, the extent of the cavity becomes apparent...the vastness thus made sensible transfixes the mind with astonishment[1]." He adds, that it was three and a half miles long, two and a half wide, and over a thousand feet deep. [The area given includes the Black Ledge half a mile wide, and inner pit.]

In a more recent account[2], Captain Dutton reports of Kilauea, "within it are the great lakes of fire always burning. The [lava] lake at the summit of Mouna Loa is frozen over and silent, without a trace of volcanic activity for several years at a time, and is open only for several months, or sometimes a year or so, before a great eruption. But at Kilauea the lava lakes are always aflame, and have been so ever since the earliest traditions of the natives. Forty years ago[3] there was a pit within a pit, and in the lowest deep was a lava pool, half a mile or more in diameter, always boiling, spouting, and flaming. At the present time the inner pit is quite filled up with solid lava, and a large conical pile of rocks is built up over the site of this former lake. Within this pile of rocks, however, is the remnant of this lake, now about ten acres in area. Half a mile distant is a second lake, which is easily visited, and it is an exhilarating sight to stand at night upon the brink of it, and watch the boiling, surging, and swirling, of six acres of melted lava. At brief intervals the surface darkens over by the formation of a black solid crust, with streaks of fire round the edges. Suddenly a network of cracks shoots through the entire crust, and the fragments turn down edgewise and sink; leaving the pool one

[1] U.S.A. Exploring Expedition, vol. IV. p. 122, 1856.

[2] "The Hawaiian Islands and People." Ordnance notes No. 343. Washington, April 23, 1884, p. 5.

[3] This must have been on the occasion of Dana's visit in November 1840, the great outbreak of lava having taken place in the previous June. See "Geology of the U.S.A. Exploring Expedition." Dana does not appear to have seen the formation of the black crust. But it was seen by Capts. Chase and Parker in 1838; and by Mark Twain, "Innocents at Home," p. 186 (quoted by Haughton, "Phys. Geog." p. 60.)

glowing expanse of exactly the appearance of so much melted cast-iron. The heat of fusion in this lake is maintained, in spite of the enormous loss of heat by radiation, by the constant ascent of large quantities of intensely hot vapours from the depths of the earth[1]."

The cycles of change in this crater may be taken to commence after an eruption, which consists of a lateral escape of lava low down in the crater, caused by hydrostatic pressure upon its gradually filling; for it never overflows. Dana has recorded notices by various visitors before his own visit in 1840. In 1838 the lower pit was full to within 40 feet of the top. There were on its surface six boiling lakes of lava, and twenty-six cones, from 20 to 60 feet high. At that time while they gazed a black crust would form, break up, and sink, as described by Captain Dutton and by Mark Twain. Two years later, and a week previous to the outbreak of June 1840 before Dana's visit in November, the whole interior was "a fearful scene of fiery deluges and ejections. The lava had overflowed the Black Ledge" (so that the inner pit was more than full) "and all was one vast theatre of intense action. Fissures then opened. The centre of the great pit sunk down 350 to 400 feet," which was its condition when visited by the U. S. A. Exploring Expedition. After this tapping of the great cauldron, the crater became comparatively inactive, and covered with a black hardened surface except where there were one or two boiling pools[2].

Captain Dutton describes the inner pit as being full[3], but subsequently on the 6th March, 1886, the lava again drained away, and the whole column sunk nearly 600 feet. On this occasion there was no eruption of the escaping lava to be seen at the surface[4]. And for a while after it no fire was visible

[1] See the Author's paper "On the Inequalities of the Earth's Surface upon the hypothesis of a liquid Substratum." Trans. Cambridge Phil. Soc. Vol. XII. Pt. II., p. 21, read Feb. 22, 1875.

[2] Dana's "Geology of the U.S.A. Exploring Expedition," and "American Journal of Science," No. 40, p. 117.

[3] Emerson, "Am. Journ. Science," vol. XXXIII., 1887.

[4] See above, and also "Hawaiian Volcanoes;" Am. Journ. Science, vol. XXV., March 1883, p. 220.

in the crater, though three months later molten lava reappeared rising at several places[1].

If we look at the bearing of the above notices upon the question of the relative densities of liquid and cooled lava, we may observe that the generally horizontal floor of the whole crater affords a presumption that it consists of a stratum of cooled lava, supported, either now or formerly, upon liquid. But the subsidence of the boiling surface of the inner pit through 400 feet in 1840, when it was "one vast theatre of intense action," and its subsequent covering with a black hardened surface except one or two boiling pools, affords a strong presumption that the newly formed lava crust must have floated. Dutton also remarks that there is abundant evidence that the floor of the pit sinks down more or less after every eruption within it[2].

When Emerson examined the "New Lake" after being entirely emptied of its liquid contents, he found a large portion of the bottom of the lake composed of huge solid boulders, as rounded and smoothed as if worn by some mountain torrent. These must have been more dense than the lava which had covered them, but from their form and size could hardly have been pieces of sunken crust. Emerson says that the lake appeared to have been connected with "the depths below" by a shaft [*qu.* semi-circular] of about ten feet radius. This looks as if the lake had not been the exposed end of a column of lava of the same sectional area as itself, but that it rather resembled the basin of a Geyser, supplied by a pipe, which seems to have been at the edge. "Near the south-western portion of the pit lay the huge bulk of the now stranded 'floating Island', a great mass of firmly cohering rock, which for more than four years had been floating like an iceberg, and slowly changing its position on the molten flood[3]." It was 60 feet high and 100 feet long. This observation is in favour of the cooled lava being the *less* dense. On the other hand we have the fact that the crust

[1] L. L. Van Slyke. "Am. Jour. Science," Feb. 1887, vol. XXXIII. p. 96.
[2] "Am. Jour. Science," *loc. cit.* 1883.
[3] *Ibid.* vol. XXXIII. p. 91, 1887.

which continually forms upon the lakes, when in their ordinary condition of activity, soon breaks up and sinks.

Mark Twain[1] graphically describes what he witnessed as follows.—"Occasionally the molten lava flowing under the superincumbent crust broke through, split a dazzling streak from 500 to 1000 feet long like a sudden flash of lightning. Then acre after acre of the cold lava parted into fragments, turned up edgeways[2], like lakes of ice when a great river breaks up, plunged downwards, and were swallowed up in the crimson cauldron. Then the wide expanse of the 'thaw' maintained a ruddy glow for a while, and became black and level again."

Captain Dutton, a highly trained observer, writes to Dana[3]: "I watched with the deepest interest the action of the lava in the lakes. The most accessible one is now called The New Lake. It undergoes a series of regular changes within a period of about two hours. When we reach the brink of it, we generally find it frozen over and quite black and still except at the edges; where we perceive a rim of fire. We observe also at many places upon the edges a little sputtering and blowing out of lava, and hear a dull simmering sound. At length a piece of the black lava upon the surface cracks, turns down its edge and sinks, disclosing a patch of livid fire. Soon after in some other part of the lake at the edge another piece breaks and goes down. This becomes more and more frequent until at last a hundred cracks suddenly shoot through the entire surface, and, with a grand commotion, numberless fragments of the frozen surface plunge downward, leaving the whole one glowing mass of lava. For a few minutes the spectacle is very grand but it does not last long. The surface quickly darkens and freezes over again, becoming black as before, and in this condition it remains for an hour or two. The period between the break-ups is not regular, being as short as forty minutes and as long as two hours and a quarter."

[1] "Innocents at Home," p. 186, quoted by Dr Haughton "Phys. Geogr." p. 60.

[2] See Dutton's account quoted at p. 47, where he uses the expression "turn down edgewise." Both these observers use the words "plunge downwards."

[3] "Am. Jour. Science," vol. xxv. p. 220, 1883.

"The explanation of the phenomenon is, I think, not difficult. When the lava first passes from the liquid to the solid condition, while its temperature is still near the melting point but below it, its density is less than that of the lava below. As the crust thickens, and the surface becomes cooler, its density becomes greater than that of the lava below, and its position then becomes unstable. A slight disturbance then produces a rupture, and the sinking of one fragment is quickly followed by that of the others."

There appears however to be an alternative supposition consistent with a less density of the crust which may explain its sinking perhaps even better, namely, that the fragments are drawn down by convection currents; not currents depending merely upon difference of temperature but upon difference of vesicularity. That such currents exist is implied in the epithets "boiling," "surging," "swirling," "wallowing[1]." The action appears to be rhythmical, otherwise a crust could not form (and did not when the action was "intense"). When after a pause it recommences "the molten lava flowing under the superincumbent crust breaks through" and the fragments are engulphed in the descending currents. This is suggested by its turning "down," or "up," "edgewise" as noticed by Dutton and Mark Twain. When a plate simply sinks in a liquid of rather less density it can scarcely be said to turn down or up edgewise for it sinks with only slight oscillations from the horizontal position. The reason clearly is that as soon as one half becomes at all depressed the pressure of the liquid upon the under side of that half is immediately increased beyond that on the other half, which in its turn is then depressed and the half which was first depressed is relatively raised, and so the two halves on each side of an axis alternately fall and rise through a small angle as the whole goes down[2].

Mr Green of Honolulu, who has observed the volcanos for many years, says of an experiment with melted stearine, "on breaking up the crust it will be seen to descend with the

[1] Miss Bird says "wallowing."

[2] This is easily illustrated on a small scale by placing a slice of a raw potato on the surface of water; the specific gravity of the potato being about 1·07.

convection currents and re-melt just as the lava crusts appear to do in the lakes of Kilauea[1]." Mr Green gives 2·90 as the density of the upper and interior masses of the Hawaiian mountains whilst the more basic and olivinistic outflows on the lower slopes have very commonly a density of 3·10; and he appears to attribute this difference to a difference in the densities of the upper and lower portions of the lava columns themselves, from which these rocks are respectively derived.

On the whole the evidence does not appear to be conclusive against, but rather in favour of, the possibility of melted lava being capable of supporting a crust of the same lava when cooled. At the same time the possibility of a crust floating may, as already remarked, depend upon an intrinsic difference in the proportions in which the constituents combine, when freezing takes place.

[1] "The Volcanic Problem." Honolulu, Aug. 25, 1884. Mr Green, however, attributes the convection currents solely to differences of temperature without the escape of vapour.

CHAPTER V.

A LIQUID SUBSTRATUM DISSOLVING GAS ACCORDING TO HENRY'S LAW MAY ACCOUNT FOR THE ABSENCE OF TIDES AT THE EXTERIOR SURFACE OF THE CRUST.

On what grounds physicists hold that the earth is rigid—Some geologists demur—Mathematical hypotheses on which the doctrine of rigidity is based—Volcanic manifestations suggest a different constitution for the interior—"Henry's law" of the absorption of gases by liquids—Application to a substratum of fused rock—Mathematical expression for the upswelling of a column of magma upon the pressure being reduced—Approximate values of densities—Quantity of water available to be given off—Application of result to modify the tides in a liquid substratum—They might be even obliterated—The same hypothesis might thus explain volcanic action and the absence of bodily tides.

From what has been stated in Chapter III., it has been inferred that, whatever be the reason, no appreciable tides are produced in the body of the earth by the attraction of the moon and sun. The conclusion drawn by physicists from this fact has been, that the earth possesses great "effective" rigidity; by which is meant that it resists the deforming influences to which it is exposed. Geologists on the other hand formerly were of opinion that the interior of the earth is liquid[1]; and although many of them have felt bound to bow to the supposed superior authority of the physicists, some of the most distinguished still adhere to the view, that, if the earth be not entirely liquid

[1] The arguments on both sides have been well epitomised by Dr Wadsworth, "On the evidence that the earth's interior is solid." *American Naturalist*, June, July and August, 1884.

within, there must be at any rate a substratum of yielding matter beneath a crust, which itself is only a few miles thick[1].

Now it is well known that mathematicians are often compelled to proceed upon hypotheses, which are at best only approximately true, and their results are to be depended upon, just so far as the defect from truth in the hypotheses is not material to the enquiry. They are on the other hand able to maintain propositions, which, apart from calculation, would hardly suggest themselves or be believed. The tidal argument for rigidity affords examples of both these principles. Apart from calculation, it would hardly have been believed, that the moon's attraction can be sufficiently powerful, so to pull a solid earth out of shape, unless it was as rigid as steel, that the change of form would, by means of the ocean tides, be made evident to proper observations. This result must be held binding nevertheless.

Under a sense of this obligation it was accepted in the first edition of this work, that the earth is on the whole extremely rigid; but it was suggested, that there might be a liquid substratum, of no great depth compared to the radius, intervening between the crust solid from cold, and the nucleus solid in spite of its high temperature from pressure[2], and that tides in this substratum would be scarcely raised on account of the viscosity of the substance, and its confinement beneath the crust[3]. That this reasoning was incorrect will appear from the observation just now made concerning the nature of viscous friction[4]. But on the other hand, with respect to the hypotheses upon which the bodily tides have been calculated, it must be borne in mind, that the substance of the earth has been assumed to be incompressible[5]; in other words the volume of a given mass of it is not to be altered by a change of pressure, but its shape only. So that, to use Professor Hennessy's words,

[1] See especially, Prestwich "On the Agency of Water in volcanic eruptions. § 6. Thickness of the earth's crust from a geological standpoint." *Proc. Roy. Soc.* No. 246, 1886.

[2] p. 26, ed. 1881. [3] p. 23, ed. 1881. [4] p. 40 *supra.*

[5] "The condition of constancy of volume of the liquid is fulfilled." Love "On the free and forced Vibrations of an elastic spherical shell containing a given mass of liquid," p. 178. *Proc. London Mathematical Soc.* vol. XIX. 1888.

what has been proved is, "That the earth does not consist of an elastic solid envelope, enclosing a mass of the ideal substance called an incompressible liquid[1]."

Supposing there to be a liquid substratum, it is probable that we shall obtain the best conception of its nature by studying volcanic phenomena.

"What is the essential character of volcanic vents? They appear from all descriptions to be orifices, from which superheated gases escape. Molten lava often fills the bottom of a crater for a long period. What keeps it hot? If it were a supply of lava from below, there would need to be a continuous escape above. But that does not appear to be requisite. It must then be the high temperature of the gases which pass through it, and in so doing support its temperature[2]." Captain Dutton, as it has been seen, endorses this view[3].

A further indication of the association of gases with fused rock deep in the earth's crust is given in the fact described by Dr A. Geikie, that amygdules occur in "hundreds of dykes" in the district examined by him in North Britain. When the rock of these dykes was in a state of fusion, the cavities represented by these can have been nothing else than gaseous vesicles[4].

Let us then examine what would be the fundamental nature of the association between the fused rock and the gas, which it is now assumed that it contains; and enquire what effect such a constitution of the magma would have upon the formation of bodily tides. The gas is supposed to consist chiefly of water above its critical temperature[5].

Now Henry's law of the absorption of gases by liquids asserts, that the volume of the gas which can be held in solution by the

[1] Quoted by Wadsworth, p. 590.

[2] From the Author's paper, "On the inequalities of the earth's surface upon the hypothesis of a liquid substratum." *Cam. Phil. Soc. Trans.* Feb. 22, 1875, p. 21.

[3] "Hawaiian Islands and People," quoted above, at p. 48.

[4] "History of Volcanic Action during the Tertiary period in the British Isles," p. 37. *Trans. Roy. Soc. Edin.* vol. xxxv. Pt. II. 1888.

[5] See the Author's paper in "Proc. Cam. Phil. Soc." vol. VI. Pt. I. read Oct 25, 1886, of which the remainder of this chapter contains the substance.

liquid is the same, whatever be the pressure. But since a given volume of gas contains, by Boyle's law, a mass proportional to the pressure, it follows, that the mass of a gas that can be held in solution by a given quantity of liquid at a constant temperature varies as the pressure.

There is no *a priori* reason why the applicability of this physical law should be confined to the ordinary range of temperatures, or why the relations between a substance, as rock, which is not liquid at a lower temperature than (say) 3000° Fah., and a substance, as water, which is not a gas under about 700° Fah., should differ essentially from those between a substance (water) and a gas (e.g. carbonic acid), which are respectively liquid and gaseous at ordinary temperatures[1]. It is quite possible that the solubility of water-gas in molten rock may be as great as that of carbonic acid in water, in which case we should have the ratio of the volumes about equal to unity. But apart from experiment, there can be no certainty upon this ratio.

We will first suppose a stratum of liquid, which, if there were no absorbed gas, would be of uniform density; and that it is subject to the action of gravity constant for all its depth. We will likewise suppose it to support a crust of the same density as the liquid. These suppositions differ from what would be the real case with our liquid substratum; for gravity would not be constant, and the density of the rocky constituent might be somewhat greater in its lower portions. But the above suppositions will greatly facilitate calculation, and will lead to a conclusion sufficiently exact for our purpose.

Let m be the volume of gas held in solution in unit volume of the liquid, γp the mass of the same under the pressure p; where m and γ are two constants, m depending upon the solubility of the particular gas in the particular liquid at the

[1] Sir I. Lowthian Bell has described the manner in which gas is absorbed by slag in the iron furnace at ordinary pressures, and given out suddenly upon the slag solidifying. ["On the occlusion or absorption of gaseous matter by fused silicates at high temperatures, and its possible connection with volcanic agency." Jour. Iron and Steel Ins. No. II. 1881.] And Captain Dutton of the U. S. Geol. Survey informs me, that lava upon solidifying excludes steam, which it has held in solution so long as it was liquid. See also Mattieu Williams "On Lunar Volcanos," Ast. Soc. Monthly Notices, XXXIII. p. 360, 1873.

given temperature, and γ depending upon the compressibility of the gas at that temperature.

If the analogy of water dissolving different gases at ordinary temperatures can be taken as a guide, it would seem that m will vary greatly for different gases, as appears from Henry's experiments. In the case of carbonic acid and water he found m to be about unity. In the case of sulphuretted hydrogen and water, m was about 0·86[1].

If the liquid is saturated with gas at all depths, the mass of the gas dissolved by a given quantity of it will be greater at greater depths on account of the increased pressure there: and if, on the pressure being relieved from that required for saturation, vesicles of gas are separated from it, these must be subject to the liquid pressure.

Let the liquid be exactly saturated at every depth ζ under the pressure ϖ at that depth. It will contain no vesicles of free gas, and if the density of the liquid is σ, the density of the mixture will be $\sigma + \gamma\varpi$. We then have to determine ϖ,

$$\varpi = g\int(\sigma + \gamma\varpi)\,d\zeta.$$

Suppose there to be a solid cooled crust of thickness k and density σ. Then

$$\varpi = \left(\frac{\sigma}{\gamma} + g\sigma k\right)\epsilon^{\gamma g\zeta} - \frac{\sigma}{\gamma} \quad\ldots\ldots\ldots\ldots\ldots\ldots(1).$$

In the earth the crust must be of less density than the

[1] It seems possible that m might be roughly determined for slag and the gases of the furnace. But if the analogy of water dissolving different gases at ordinary temperatures can be taken as a guide, it would seem that m will vary greatly for different gases, for it appears from Henry's experiments ("Phil. Trans. Roy. Soc. 1803") that,

100 cubic inches of water at 55° Fah. absorbed of

Carbonic acid about	100	or $m=1$
Sulphuretted hydrogen	86	,, $m=0{\cdot}86$
Nitrous oxide	54	,, $m=0{\cdot}54$
"Nitrous gas"	5	,, $m=0{\cdot}05$
"Oxygenous gas"	2·63	,, $m=0{\cdot}0263$
Phosphuretted hydrogen	2·14	,, $m=0{\cdot}0214$
"Gaseous oxide of carbon"	2·01	,, $m=0{\cdot}0201$
Carburetted hydrogen	1·40	,, $m=0{\cdot}0140$
"Azotic gas"	1·20	,, $m=0{\cdot}012$
Hydrogen gas	1·08	,, $m=0{\cdot}018$

substratum, being probably about 2·68 (that of granite); while $\sigma = 2{\cdot}96$ (the density of basalt). The false assumption can be corrected by giving a compensatory less value to k.

Next suppose that the pressure upon that portion of the mixture where it was ϖ becomes p, being less than ϖ. Some of the gas will then be liberated; and by that means let the element $d\zeta$ become dz. The volume of the gas which was wholly dissolved in $d\zeta$ was $md\zeta$. In dz the total volume of the gas, which will be partly free and partly dissolved, being inversely as the pressure, will be

$$m\frac{\varpi}{p}d\zeta.$$

Of this the volume dissolved remains by Henry's law unaltered, *viz.* $md\zeta$, so that the volume of free gas in dz will be

$$m\frac{\varpi}{p}d\zeta - md\zeta.$$

This will be the volume by which the element is expanded by change of pressure from ϖ to p: so that

$$dz = d\zeta + m\frac{\varpi}{p}d\zeta - md\zeta.$$

But since the amount of matter in the column is the same identically as before, p can differ from ϖ only by the difference of the surface pressures. Suppose the surface pressure corresponding to p to be $g\sigma c$, we then get the value of p from that of ϖ by subtracting $g\sigma k$ and adding $g\sigma c$ in its place, *i.e.* by subtracting $g\sigma(k-c)$; whence, substituting for ϖ from (1),

$$dz = \left\{1 + \frac{mg\sigma(k-c)}{\left(\frac{\sigma}{\gamma} + g\sigma k\right)\epsilon^{\gamma g\zeta} - \frac{\sigma}{\gamma} - g\sigma(k-c)}\right\}d\zeta.$$

Observing that z and ζ begin together, we obtain by integration,

$$z - \zeta = \frac{m(k-c)}{1+\gamma g(k-c)}\log_\epsilon\left\{1 + \frac{(1+\gamma g(k-c))(1-\epsilon^{-\gamma g\zeta})}{\gamma gc}\right\},$$

where $z-\zeta$ is the upswelling of the column caused by the surface pressure being reduced from $g\sigma k$ to $g\sigma c$.

For the purpose of making an approximate application of this result to a liquid substratum, we will consider the order

of magnitude of the product $\gamma g\zeta$, which expresses the mass of gas that would be dissolved in unit volume of the magma under a pressure equal to that of a column of the standard substance (viz. water) of the height ζ.

We have seen that the density where the pressure is ϖ will be $\sigma + \gamma\varpi$. If then we put for ϖ its value from (1), we find under the pressure of a column of the saturated magma, including the pressure of the crust, at the depth ζ from the bottom of the crust,

$$\gamma g\zeta = \log_\epsilon \frac{\sigma'}{\sigma} - \log_\epsilon (1 + \gamma gk) \text{..............(2)};$$

$\sigma' - \sigma$ being the increase of density between the top and bottom of the substratum, due solely to the absorbed gas. If the rocky matter were homogeneous, this might be true: but possibly the density may be intrinsically greater in the lower portions. In that case $\gamma g\zeta$ would be less than the value of the above expression, when the actual values of the density are substituted for σ' and σ.

It is impossible to know the exact values of σ' and σ, but we may assume that they do not differ materially from what Laplace's law would give, and the rather, because we require only their ratio. The diagram[1] shows that the density increases approximately as the depth near the surface; whence, the surface density being taken as 2·750, and the density at one tenth of the radius 3·882, we get 0·00286 as the increase per mile. If then we take the thickness of the crust to be 25 miles, and the depth of the substratum 75 miles, we find at the bottom of the crust

$$\sigma = 2{\cdot}815;$$

and at the bottom of the substratum

$$\sigma' = 3{\cdot}036;$$

whence $\log_\epsilon \dfrac{\sigma'}{\sigma} = 0{\cdot}0733$. And $\gamma g\zeta$ is less than this, and *a fortiori* γgk is less. Hence if we put $\zeta = nk$, we find from (2) generally for depths of about 100 miles from the surface

$$\gamma gk = \frac{1}{1+n} \log_\epsilon \frac{\sigma'}{\sigma},$$

[1] p. 35.

and for $k = 25$ miles and $\zeta = 75$ miles we have $\gamma g k = 0{\cdot}0183$ about.

If we knew this to be the value of $\gamma g k$, it would imply that the gas dissolved at the bottom of the crust is 0·0183 of the mass of the liquid rock which holds it in solution. Its mass within a unit mass of rock will therefore be $0{\cdot}0183 \times \sigma$; say $0{\cdot}0183 \times 2{\cdot}82$ or 0·0516 of the standard substance, which is liquid water. At this rate one cubic foot of magma, just under the crust, would yield 89 cubic inches of liquid water. All that we can assert is that these numbers are probably of the proper order of magnitude, and if so they suffice to show that the present theory would account for the emission of a very considerable quantity of steam from the substratum if it gained access to the atmosphere.

In the expression for $z - \zeta$, substitute for $\epsilon^{-\gamma g \zeta}$ its equivalent from (2), viz. $\frac{\sigma}{\sigma'}(1 + \gamma g k)$, and it can then be put into the form

$$z - \zeta = \frac{m(k-c)}{1 + \gamma g(k-c)} \log_\epsilon \left\{ 1 + \frac{1}{\gamma g c} \cdot \frac{\sigma' - \sigma}{\sigma'} - \frac{\sigma}{\sigma'} \frac{k}{c} + \frac{k-c}{c} \left(1 - \frac{\sigma}{\sigma'}(1 + \gamma g k) \right) \right\}.$$

But if $k - c$ is very small compared with k or c, c may be put equal to k in those terms which are multiplied by $k - c$. Also $\gamma g (k-c)^2$ may be neglected. We then obtain as an approximate expression for the upswelling of the column, owing to the superincumbent pressure being reduced from $g\sigma k$ to $g\sigma c$,

$$z - \zeta = m(k-c) \log_\epsilon \left\{ \frac{\sigma' - \sigma}{\sigma'} \left(1 + \frac{1}{\gamma g k} \right) \right\}.$$

If now we suppose a still further quantity of gas to be liberated owing to the surface pressure being further slightly reduced from $g\sigma c$ to $g\sigma c'$, and that the height of the column of magma then becomes z', we have, by substituting z' for z and subtracting,

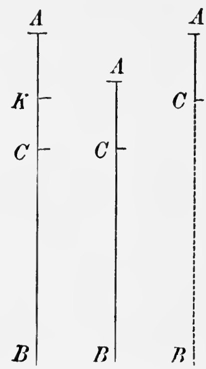
$$z' - z = m(c - c') \log_\epsilon \left\{ \frac{\sigma' - \sigma}{\sigma'} \left(1 + \frac{1}{\gamma g k} \right) \right\}.$$

The upswelling of the column is consequently sensibly proportional to $(c - c')$, which is the diminution of the superincumbent

pressure; and this is true, whether there be already vesicles or not, so long as the rocky matter is saturated with gas.

The doctrine of the entire solidity of the earth rests chiefly, as has been explained, upon the alleged fact that no appreciable tides can exist in the interior of the globe, such for instance as would be formed in a substratum of liquid freely intercommunicating throughout a layer of the sphere, provided the liquid were, as is usually assumed, incompressible. But the constitution of the magma now suggested renders it possible to account for the absence of measurable tides at the surface of the cooled crust.

Let AK be the thickness of the solid crust, KB the depth of the substratum at high tide therein; and suppose that the rocky matter is completely saturated with gas whether there be free gas among it or not. Let KC be the small space through which the tide might fall, if the column consisted of inexpansible liquid.



(1) (2) (3)

This fall would be caused by the difference, or withdrawal, of an amount of liquid represented by KC within the column.

If then we neglect the weight of the small amount of gas contained in KC, the pressure upon the column CB will be lessened by $g\sigma KC$ (see fig. 2). The effect would be appreciably the same as if the substratum supported a *crust* of the thickness AC in (1) at one time, and of the thickness AC in (2) at the other time; that is the pressure of the additional liquid in the column may be regarded as arising in (1) from an addition to the crust instead of to the column itself; so that KC represents the quantity $(k-c)$, while CB in (1) and (2) represents ζ, and CB in (3) represents z; and the magma in (2) expanding on account of the withdrawal of KC, we shall have by our formula,

$$\textit{expansion of } BC = mKC \log_\epsilon \frac{\sigma' - \sigma}{\sigma'} \left(1 + \frac{1}{\gamma g k}\right).$$

If then the expansion of the column CB, owing to the

relief of pressure caused by the withdrawal of the liquid in KC, happened to be equal to KC, as in (3), the surface of the substratum would remain at the same level as before, and no tide would be apparent at the upper surface of the crust. Now the formula shows that with a certain relation among the constants this might happen: for if m, which measures the volume of the gas absorbed according to Henry's law, was the reciprocal of $\log_\epsilon \left\{ \frac{\sigma' - \sigma}{\sigma'} \left(1 + \frac{1}{\gamma g k}\right) \right\}$, we should have by our formula,

$$\textit{expansion of } BC = KC,$$

i.e. the level of the point A would be unaltered. Using Laplace's law of density as has been done before, this would be the case if the thickness of the crust was 25 miles, and the depth of the substratum 50 miles, and $m = 0{\cdot}88$; or if the depth was 75 miles, and $m = 0{\cdot}73$; or if the depth was 100 miles, and $m = 0{\cdot}62$. These numbers show that, if the solubility of the gas in molten rock was analogous to that of carbonic acid in water, or of sulphuretted hydrogen in water; and the depth somewhere between 50 and 100 miles; scarcely any tide affecting the level of the surface would be raised in the substratum, for the effect would consist merely of a change in the vesicularity of the magma following the tide-raising body.

There would however be produced, what might be called a density tide in the substratum[1]. Now a change of density in a purely gaseous medium beneath the crust would imply a change of pressure upon its under side, alternately raising and depressing it, and thus causing a tide in it. This would no doubt occur if the substratum was subject merely to Boyle's law. But the effect of Henry's law would be more complicated, and our investigation shows that, under certain circumstances, we might have a change of mean density within the substratum, without an appreciable change of pressure being thereby

[1] There would seem, from Prof. Palmieri's observations, to be some connection of the eruptive phenomena of Vesuvius with internal tides. See Phillips' "Vesuvius," p. 171, 1869.

produced beneath the superincumbent crust, so as to raise or depress it, thereby causing a tidal change of level at its surface.

The vesicles of gas liberated during the passage of the tide producing body being extremely minute would not have time to rise through the liquid before they were redissolved by the recurring pressure.

It appears therefore that, although the numerical values made use of in the foregoing are subject to a considerable margin of error, they must be sufficiently near the truth to show, that the present hypothesis may possibly account for the alleged absence of bodily tides with a liquid substratum constituted as we have imagined, and thus remove the principal argument against its existence.

If it be true that the two seemingly independent phenomena, namely the large escape of water and other gases at a high temperature from volcanos, and the absence of bodily tides within the earth, can be explained by a single hypothesis, this affords a decided presumption in its favour.

It does not appear necessary that we should concern ourselves with the further question, whether the cooled crust ought to exhibit tides within its own thickness, whereas in fact it does not. All that we desire to do is, to establish the physical possibility of the presence of a fused liquid substratum beneath it, the existence of which is rendered probable by geological considerations.

CHAPTER VI.

A THIN CRUST IMPLIES AN ENERGETIC SUBSTRATUM.

Sir W. Thomson "On the Secular Cooling of the Earth"—His view based upon the supposed necessity of solidity—This has been shown in the preceding chapters to be not certain—His mathematical expression for the temperature within the crust adapted to the case of a crust resting upon an inert liquid substratum—The result proves that, on the hypothesis of a thin crust, the substratum cannot be inert—But must be affected by convection currents—Bearing of this conclusion upon wide areas of oscillations of level.

THE paper by Sir W. Thomson "On the Secular Cooling of the Earth[1]" is of great interest and well known. From his mode of viewing the manner of its occurrence, he believes that the earth passed from the state of a liquid to that of a solid globe in a comparatively short space of time; since which period all geological events have happened. His formula expresses the rate of increase of temperature at any given depth in terms of the length of time since complete consolidation took place, and the conductivity of rock. The resulting curve of temperature is of such a form that, for reasonably large values of the time in question—or of the age of the habitable world—and for all such depths as the puny efforts of man can reach, it would be impossible to perceive at the

[1] "Trans. Roy. Soc. Edinburgh," vol. XXIII. Pt. I. p. 157; also "Phil. Mag.," 4th Series, vol. XXV. p. 1, 1863, and Thomson and Tait's "Treatise on Nat. Phil.," Appendix D.

present day any deviation from a uniform rate of increase; or the curve of temperature is a straight line. But if it were possible to reach very great depths, the rate would be found to be sensibly diminishing; and even at the centre of the earth a greater temperature need not exist than what the uniform rate, which obtains at the surface, would bring us to at about sixty miles. The greater the value we assign to the presumed age of the world, the deeper we should have to dig before we should find the rate sensibly diminishing. Some interesting particulars concerning the relation between different assumed values for the world's age, and the depths at which it might be practicable to detect a diminution in the rate of increase of temperature, will be found in Sir W. Thomson's sectional address to the British Association, 1876.

His general conclusion is that "the earth, although once all melted, or melted all round its surface, did, in all probability, really become a solid at its melting temperature all through, or all through the outer layer, which had been melted; and not until the solidification was thus complete, or nearly so, did the surface begin to cool[1]." The conclusion at which he arrives on these grounds, respecting the present distribution of temperature, taking 7000° F. as the temperature of solidification, is that the rate of increase from the surface downwards would be sensibly $\frac{1}{51}$ of a degree F. per foot for the first 100,000 feet (19 miles) or so. Below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet (76 miles) it would have diminished to about $\frac{1}{141}$ of a degree per foot, and so on rapidly diminishing as shown in the curve[2]. "Such is on the whole the most probable representation of the earth's present temperature, at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles; below which the whole mass is, whether liquid or solid, probably at or very nearly at the proper melting temperature for the pressure at each depth"—that is to say, the cooling process not having at present reached so far, the state of the matter there cannot in any way influence the law of temperature in the strata above.

[1] "Natural Philosophy," Appendix D. [2] *Ibid.*

It must be borne in mind that Sir W. Thomson bases this "probable representation of the earth's present temperature" upon the assumption that it is solid; an assumption which appears to many geologists to be at variance with observed facts. In the preceding chapter we think we have shown, that the chief argument for solidity may possibly be met by a hypothesis concerning the constitution of the interior on other grounds highly probable. We now proceed to enquire, whether any information may be gained regarding the condition of the interior, by considering its relation to the temperature of the crust.

Taking the most general form of Sir W. Thomson's expression for the temperature within the crust, neglecting, as he has done, the sphericity, we will adapt it to the case of a crust, which is being formed by freezing out of a subjacent reservoir full of the same substance as the crust, but in a state of liquid fusion.

The following symbols are used:

v = the temperature at the depth x at the time t.

t = the time since a crust began to be formed; the unit of time being one year.

V = the temperature of solidification (or incipient fusion) of rock.

κ = the conductivity of the rock composing the crust, expressed in terms of rock; *i.e.* κ = the numbers of units of heat which would pass across one square foot of a plate of rock one foot thick in one year; the two faces of such plate being maintained at temperatures differing by one degree Fah., and the unit of heat being the amount requisite to raise one cubic foot of rock through one degree Fah. Sir W. Thomson has deduced the value 400 for κ from observations on several kinds of rock *in situ.*

λ = the latent heat of molten rock measured by the same units.

Λ = the latent heat measured by thermal units centigrade, water being the standard substance.

k = the thickness of the cooled crust at the time t.

β = the temperature rate at the surface of the earth.

$$\mathrm{M} = \int_0^\mu \epsilon^{-z^2} dz.$$

Then the general form of the equation used by Sir W. Thomson, to express the temperature v at the depth x, would be,

$$v = A + \phi \int_0^x \epsilon^{-\theta x^2} \, dx;$$

where ϕ and θ are functions of t only.

This equation must satisfy the differential equation for the conduction of heat in one dimension in a solid, viz.,

$$\frac{dv}{dt} = \kappa \frac{d^2 v}{dx^2}.$$

It is not difficult to show that this leads to values of ϕ and θ such that,

$$v = A + \frac{B}{\sqrt{4\kappa t + C}} \int_0^x \epsilon^{-\frac{x^2}{4\kappa t + C}} \, dx.$$

And we have to determine the constants A, B, and C, consistently with the conditions of our problem.

For all values of t we have $v = A$ when $x = 0$. Therefore A is the supposed constant temperature of the surface. For simplicity we may regard the surface temperature as the zero of our thermometer, and then $A = 0$,

and $$\frac{dv}{dx} = \frac{B}{\sqrt{4\kappa t + C}} \epsilon^{-\frac{x^2}{4\kappa t + C}}.$$

Now, as soon as a crust begins to be formed, its upper surface will be at the temperature of the atmosphere (nearly), and its under surface at the temperature of solidification. There will then be a finite difference of temperature in an infinitely small interval of depth: in other words, $\frac{dv}{dx}$ must be infinite when $t = 0$. This requires that $C = 0$, and the equation becomes,

$$v = \frac{B}{\sqrt{4\kappa t}} \int_0^x \epsilon^{-\frac{x^2}{4\kappa t}} \, dx \text{ ....(1).}$$

It now remains to determine B.

At the bottom of the crust we have the melting temperature.

$$\therefore \quad V = \frac{B}{\sqrt{4\kappa t}} \int_0^k \epsilon^{-\frac{x^2}{4\kappa t}} dx$$

$$= B \int_0^{\frac{k}{\sqrt{4\kappa t}}} \epsilon^{-\frac{x^2}{4\kappa t}} d\left(\frac{x}{\sqrt{4\kappa t}}\right) \quad \text{.................. (2).}$$

For $\frac{x}{\sqrt{4\kappa t}}$ write z.

Then when $x = k$, $z = \frac{k}{\sqrt{4\kappa t}}$.

Now κ, when measured as defined above, is of dimensions $\frac{L^2}{T}$; therefore $\frac{k^2}{4\kappa t}$ must be numerical. Suppose then that

$$\frac{k^2}{4\kappa t} = \mu^2 \quad \text{.......................... (3).}$$

Then μ^2 cannot contain κ, for if it did it would be of dimensions in L. And if it does not contain κ, it cannot contain t, otherwise it would be of dimensions in T. It follows that μ is a constant, which would depend upon the ratio of V to λ, which are the only remaining quantities involved in the problem of an inert liquid freezing, and are both of the dimensions of the unit of temperature.

Returning to the equation (2) we see that, when

$$x = k, \; z = \frac{k}{\sqrt{4\kappa t}} = \mu.$$

Accordingly the equation may be written

$$V = B \int_0^{\mu} \epsilon^{-z^2} dz,$$

where, as we have seen, μ is a constant depending on $\frac{V}{\lambda}$.

If μ is known, the value of the definite integral can be found in the tables, and calling it M, we have to determine B,

$$V = BM \quad \text{.............................(4).}$$

We now proceed to find μ.

Fixing the attention on the bottom of the crust, we find it at the temperature V, and solid. The liquid which underlies it is also at the temperature V. Therefore no heat can flow into

the crust owing to difference of temperature. But upon a layer dk solidifying, its latent heat is given up, so that the bottom of the crust may be looked upon as a source of heat; not indeed raising the temperature, but delaying its fall; and an amount of heat $\lambda \frac{dk}{dt} dt$ may be regarded as flowing upwards from the bottom of the crust in the interval dt.

Now the heat which flows there is expressed by

$$\kappa \frac{dv}{dx_{=k}} dt \qquad \text{(5).}$$

Hence we have

$$\int_0^t \lambda \frac{dk}{dt} dt = \int_0^t \kappa \frac{dv}{dx_{=k}} dt.$$

$$\therefore \quad \lambda k = \int_0^t \kappa \frac{B}{\sqrt{4\kappa t}} \epsilon^{-\frac{k^2}{4\kappa t}} dt$$

$$= \int_0^t \frac{\kappa B}{\sqrt{4\kappa t}} \epsilon^{-\mu^2} dt$$

$$= B\sqrt{\kappa t}\, \epsilon^{-\mu^2}.$$

And by (3)
$$\sqrt{t} = \frac{k}{2\mu\sqrt{\kappa}},$$

whence
$$B = 2\mu\epsilon^{\mu^2}\lambda \qquad \text{(6).}$$

But by (4)

$$V = BM;$$

$$\therefore \quad V = 2\mu\epsilon^{\mu^2}\lambda M.$$

And
$$\mu\epsilon^{\mu^2} M = \frac{V}{2\lambda} \qquad \text{(7).}$$

This equation gives μ in terms of the melting temperature and the latent heat. B can then be expressed in terms of the same constants, and the problem may be considered solved as far as regards the freezing of an inert liquid, for which the temperature of fusion and the latent heat are known[1].

[1] In this form the investigation would be at once applicable to the case of a crust of ice forming upon water contained in a vessel impervious to heat.

It may be asked what the equation (1) would mean, if values were given to x greater than k, or extending into the region of liquidity.

The answer appears to be, that v would express the temperature which would

Since by (3) $\sqrt{4\kappa t} = \frac{k}{\mu}$, the equation may be written

$$v = \frac{\mu}{k}\frac{V}{M}\int_0^x \epsilon^{-\frac{x^2}{4\kappa t}}\,dx.$$

This shows that at the time t, when the thickness of the crust is k, the temperature rate β at the surface, where $x = 0$, is $\frac{V}{k}\frac{\mu}{M}$. If therefore we express k in miles, seeing that β is expressed in degrees per foot, we have,

$$k = \frac{V}{\beta}\frac{\mu}{M}\frac{1}{5280}.$$

The temperature rate at the surface is put at $\frac{1}{51}$, but sometimes lower. We shall not err much by putting it at $\frac{1}{52{\cdot}80}$, and so we may say that roughly,

$$k = \frac{V}{100}\frac{\mu}{M}.$$

Now μ is always greater than M.[1]

$$\therefore \quad k > \frac{V}{100},$$

or the thickness of the crust when expressed in miles is numerically greater than one hundredth of the melting temperature expressed in degrees Fah., provided the thickening goes on uninterruptedly.

It will be necessary to form some opinion about the temperature of solidification. Siemens found the temperature at which platinum melts to be 3272° Fah. Mallet, in his experi-

exist in an infinite solid at that depth, under the conditions that the temperature of the surface is zero, and that the flow of heat at the depth k is equal to $\lambda\frac{dk}{dt}$; a quantity which varies inversely as the square root of the time. It will be seen that this condition would affect the temperature rate in an infinite solid below, as well as above, that depth. But such a downward extension of the solid does not exist, and need not concern us.

[1] The values of $\int_0^T \epsilon^{-t^2}\,dt$ to ten places of decimals for values of T from 0 to 4·52 are given in Oppolzer's Lehrbuch zur Bahnbestimmung der Kometen und Planeten, Zweiter Band, Tafel x. Leipzig, 1880. From $T=4{\cdot}52$ to $T=\infty$ there is no change in the first ten places of decimals.

ments upon the contraction of slags, concluded that they began to solidify at about 3000° Fah. Since platinum is a far more refractory substance than slag, this estimate must have been excessive. Let us then place the temperature of solidification at 2550° Fah. Then taking the surface temperature at 50°, we shall have $V = 2500°$.

When we say that the latent heat of rock is λ, we mean that λ is the ratio of the quantity of heat, required to melt unit volume at the temperature of fusion, to the quantity required to raise unit volume of solid rock through one degree Fah. But if we neglect the change of density on fusion, each of these units of volume contains the same number of units of mass. Hence λ will also be the ratio of the quantity of heat, required to melt unit mass of rock, to that required to raise unit mass of solid rock through one degree Fah.; or through 100/180 of a degree centigrade; or to raise unit mass of water through s100/180 C., s being the specific heat of rock. If then in the centigrade system Λ is the ratio of the quantity of heat, required to melt unit mass of rock, to that required to raise unit mass of water through one degree centigrade,

$$\therefore \quad \Lambda = s\lambda \frac{100}{180}.$$

Acccording to Mallet's determination, the mean value of s is 0·199[1],

$$\therefore \quad \Lambda = 0{\cdot}110\dot{5}\lambda.$$

Putting $V = 2500°$ Fah., we obtain from the relation given in (7),

$$\lambda = \frac{2500}{2\mu\epsilon^{\mu^2}M}.$$

And if we give to μ the values 0·66, 1, 2, 3, we find the corresponding values of λ and Λ as below.

μ	λ	Λ
0·66	2128	235·262
1·00	615·74	68·073
2·00	12·977	1·434
3·00	0·058	0·006

[1] For 16 kinds of rock as grouped by Mallet; "On Volcanic Energy," § 132. "Phil. Trans." vol. CLXIII. 1873.

The latent heat of water, which is the highest known, is 79·25° C. and the lowest of those given in B. Stewart's list[1] is that of mercury, viz. 2·83° C. There can be little doubt therefore that Λ for rock must lie between the values corresponding to $\mu = 1$ and $\mu = 2$. Wherefore μ must lie between 1 and 2.

Now $$\mu = \frac{k}{\sqrt{4\kappa t}}.$$

And if we put $k = 25$ miles, and m the number of millions of years in t, then

$$\mu = \frac{25 \times 5280}{\sqrt{4 \times 400m \times 10^6}} = \frac{3·3}{\sqrt{m}}.$$

Hence, if we assume the time to be 25 millions of years, μ will be 0·66. If the time is taken longer, μ will be smaller. The corresponding values of the latent heat would be so large as to be inadmissible. Hence we see that the hypothesis of a crust of 25 miles thick resting on an inert liquid substratum of molten rock is untenable, if the time has been as long as 25 millions of years since solidification commenced.

Let us now see what the time would be for such values of μ as are consistent with the limits within which the latent heat must lie. We have found that μ must lie between 1 and 2.

If $\mu = 1$, $m = 10·89$.

If $\mu = 2$, $m = 2·72$.

Hence, if the crust is 25 miles thick, and if it rests on an inert liquid substratum, it must have begun to solidify somewhere between 11 millions and 3 millions of years ago. The time cannot have been so short as this.

If the crust was 50 miles thick, the value of m would have been between 43·56 and 10·88, or the time between about 44 and 11 millions of years. But a crust of the average thickness of 50 miles is much greater than our previous estimates make it, while the periods corresponding to it are still decidedly less than geological considerations would assign to the age of the world.

The conclusion to which we are therefore led is, that, on the

[1] "Heat," p. 291, 1871.

hypothesis of a thin crust, the liquid substratum on which it rests cannot be inert.

Let us now consider the case of an energetic substratum. We may then suppose that in the interval dt, during which, if the substratum were inert, a certain thickness would have been frozen on to the bottom of the crust owing to the latent heat belonging to it being conducted away, instead of that, a thickness $\frac{dk}{dt}dt$ only is frozen, while in the same time, owing to the action of the substratum, a thickness $\frac{dy}{dt}dt$, which would otherwise have additionally been frozen, is prevented from freezing. Then we may regard the result to be the same as if

$$\frac{dk}{dt}dt + \frac{dy}{dt}dt$$

was frozen, and $\frac{dy}{dt}dt$ afterwards melted off. But upon

$$\frac{dk}{dt}dt + \frac{dy}{dt}dt$$

being frozen, a corresponding quantity of latent heat passes into the crust at the depth k[1]. We therefore must have, by (5),

$$\lambda\left(\frac{dk}{dt} + \frac{dy}{dt}\right)dt = \kappa\,\frac{dv}{dx_{=k}}\,dt.$$

If we then assume that in this case also $k = \mu\sqrt{4\kappa t}$, where μ is constant, we shall have as before

$$\lambda\,(k + y) = B\epsilon^{-\mu^2}\sqrt{\kappa t} \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots (8).$$

[1] Looking at this another way, the elementary layer $\frac{dy}{dt}dt$ is kept liquid by heat which it absorbs from the substratum. Why does it need this heat? The reason can only be, because the heat $\lambda\frac{dy}{dt}dt$ has flowed away through the bottom of the crust, in addition to that which has been parted with by the layer $\frac{dk}{dt}dt$ which has become solidified. Hence the total heat which has flowed there in the interval dt is $\lambda\left(\frac{dk}{dt}dt + \frac{dy}{dt}dt\right)$ as in the text.

And $\sqrt{t}$ being $= \dfrac{k}{2\mu\sqrt{\kappa}}$,

$$\therefore \quad \lambda\left(1 + \frac{y}{k}\right) = \frac{B}{2\mu\epsilon^{\mu^2}},$$

$$\therefore \quad \lambda = \frac{V}{2\mu\epsilon^{\mu^2} M\left(1 + \dfrac{y}{k}\right)} \quad \text{................(9).}$$

It will be remembered that in the case of an inert substratum it was certain that μ was constant. In this case however it is an assumption. But it is one which must be made, otherwise B will not be constant, and the solution of the differential equation will not hold good. The assumption is justified if $\dfrac{y}{k}$ is a constant ratio at all times, as appears by (9); that is to say, the quantity melted off must bear a constant ratio to the increase in thickness.

Differentiating (8), and putting α and γ for constants,

$$\frac{dk}{dt} + \frac{dy}{dt} = \frac{\alpha}{\sqrt{t}}.$$

$$\text{But } \frac{y}{k} = \gamma, \text{ gives } \frac{dy}{dt} = \gamma\frac{dk}{dt},$$

$$\therefore \quad \frac{dk}{dt}(1 + \gamma) = \frac{\alpha}{\sqrt{t}}.$$

Also $$\frac{dy}{dt}\left(1 + \frac{1}{\gamma}\right) = \frac{\alpha}{\sqrt{t}}.$$

Hence both $\dfrac{dk}{dt}$ and $\dfrac{dy}{dt}$ vary as $\dfrac{1}{\sqrt{t}}$, and the supposition that μ is constant implies that the melting off, as well as the concomitant thickening, is greater in the early stages of the formation of the crust. This seems as reasonable an hypothesis as can be made concerning the action of the substratum, about which we know nothing, except that the energy must decay as it loses heat, which is in accordance with the hypothesis; and unless some assumption is made the equation cannot be solved.

The assumption just made, that in this case also $\mu = \sqrt{4\kappa t}$, where μ is constant, gives as before, if the crust is 25 miles thick,

$$\mu = \frac{3{\cdot}3}{\sqrt{m}}.$$

So that, if the time since solidification commenced has been 25 millions of years, $\mu = 0{\cdot}66$; and if the time has been 100 millions of years, $\mu = 0{\cdot}33$.

If we put $\mu = 0{\cdot}66$, or the time 25 millions of years, then

$$\frac{V}{2\mu\epsilon^{\mu^2}M} = 2128,$$

and

$$\lambda = \frac{2128}{1 + \frac{y}{k}}.$$

And if $\mu = 0{\cdot}33$, or the time has been 100 millions of years, then

$$\lambda = \frac{10669}{1 + \frac{y}{k}}.$$

If we knew the latent heat which the liquid magma would yield up on passing into the solid rock of the crust, then, for any assumed age of the world, the expression (9) would afford the means of comparing the rapidity with which the bottom of the crust is melted off, with the balance of thickening which survives the melting.

As for the rocky constituent of the magma, its latent heat (Λ) is almost certainly considerably less than that of water, which is 79·25° C. We may fairly put it at less than 55° C. But there is the supposed gaseous constituent of the magma to be considered. Taking this to consist of water gas, the temperature at which the magma would solidify would still be much above the critical temperature of water. The water would therefore remain gaseous, and the solidifying magma could not retain it in solution, but would, according to the observations of Sir I. L. Bell on slags, and of Captain Dutton on lavas, extrude

it[1]. This is not the place to consider what would become of that gas, but it seems clear that the latent heat given up by the magma on solidification would be only that belonging to the rocky, irrespective of the gaseous, constituent.

We may conclude then that 500° Fah. for λ, which corresponds to 55° C. for Λ, would be an excessive value for the latent heat; while if we take 25·6° Fah. for λ, which corresponds to 2·83° C. for Λ (the latent heat of mercury), λ would be too small[2].

The values of the ratio y/k, corresponding to these two limiting values of the latent heat, would be, for the two periods of 25 millions of years and 100 millions of years, as recorded in the annexed table.

CRUST 25 MILES THICK.

Time	μ	Λ (latent heat cent.)	λ	$\frac{y}{k}$
25 mil.	0·66	55·28	500	3·25
—	—	2·83	25·6	82·1
100 mil.	0·33	55·28	500	20·34
—	—	2·83	25·6	415·75

The time is taken at 25 millions of years and at 100 millions of years.

μ is a constant, depending on the whole time and the conductivity of rock.

Λ latent heat centigrade, referred to water as the standard substance.

λ latent heat Fahrenheit, referred to rock as the standard substance.

$\frac{y}{k}$ the ratio of melting off to thickening at the bottom of the crust, supposed a constant ratio during the whole time.

[1] p. 56, note.

[2] It must be borne in mind that λ refers to rock and Λ to water as the standard substance of comparison.

The table given above shows that, if the time since solidification commenced is as great as 100 millions of years, the remelting must go on very rapidly compared with the balance of freezing, especially if the latent heat of rock is very low, approaching that of mercury. If the time has been less, the rate of remelting need not be so rapid. It would be very desirable to know the latent heat of rock. But in any case it seems quite certain that the amount of remelting is very great compared with the balance of freezing which remains, and by which the crust has been built up in the course of ages. Now the only way, in which this remelting can be accounted for, is by a quantity of heat coming up from below.

We have thus arrived at what appears to be a most important conclusion regarding the interior of the earth on the hypothesis of a thin crust. We have seen that, not only might a crust begin to be formed before convection ceased, but that, even now, there must be convection of heat going on: for convection it must be, because conduction of heat in a liquid could not so far exceed the conduction in the same substance when frozen, as to make the balance of freezing so much less than the remelting. Such convection implies upward and downward currents, and resulting local alterations of temperature and level, according to the play of the currents at different times; because convection is an action depending upon slight disturbing causes, so that the word "play," though not strictly scientific, well describes the changes in the motion of the liquid. Here then we find a key to the ever varying changes of level in the earth's crust; elevation over extensive areas affecting sometimes one part of the surface, and sometimes another. It appears that Humboldt long ago imagined the cause of alterations of level to be in some way connected with internal movements of a liquid interior[1].

[1] "Geological reasons render it not improbable that accidental alterations which may take place in the molten materials in the interior of the earth, easily mobile notwithstanding the great pressure to which they are subject, may cause internal displacements of mass, which may modify, after very long intervals of time, the geometric surface itself in the curvature of the meridians and parallels within small distances." Cosmos, vol. IV. p. 19, Sabine's translation.

It is unnecessary to point out the close connection of the results obtained in the present chapter with the hypothesis of the applicability of Henry's Law to a liquid substratum discussed in the preceding one.

The extreme slowness, with which the accretion of fresh solid matter at the bottom of the crust takes place, would lead us to expect that its constitution would from the first be crystalline, and not vitreous. In this respect it would differ from erupted igneous rocks, which are in some cases, at first vitreous, and in which a crystalline structure is induced by subsequent metamorphism.

CHAPTER VII.

LATERAL STRESSES AND RESULTING INEQUALITIES OF SURFACE.

Generally received views regarding the contortion of rocks by lateral pressure—Calculation of the possible amount of such pressure at the surface—The question proposed whether lateral pressure arising from contraction of the interior can account for mountain chains—Distinction between compression and contraction—Geometrical relation between compression, elevations, and depressions—Modified in the case of a solid earth—Definition of datum level of the surface—Elevations once formed by compression permanent in quantity—Datum-level equation modified to suit the case of horizontal contraction—Amount of mean depression so produced.

THE well-known fact that great lateral compression has affected the stratified rocks of the earth's crust, not once, but again and again from the earliest to the latest of the extended periods of Geological time, has been generally explained by the supposition that the globe has contracted through secular cooling[1]. Without enquiring whether the whole globe did or did

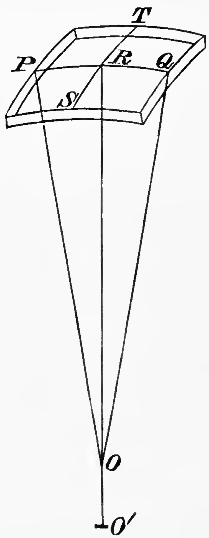
[1] The earliest publication of this theory appears to have been in a foot-note to a memoir on the mountains of L'Oisans ("Mémoires de la Société d'Histoire Naturelle," t. v. 1829) by M. Elie de Beaumont. The passage is quoted by Prof. A. de Lapparent, in a paper entitled "L'origine des inégalités de la surface du globe," in the "Revue des Questions Scientifiques," Juillet, 1880.

Prof. Sedgwick proposed the same theory two years later, in his paper on the structure of the Cumbrian mountains, "Trans. Geol. Soc.," Jan. 5, 1831. It occurred also to the Author, as he believes independently, about 1841, and was suggested to him by the wrinkles formed on old ice, when the surface of the water had sunk away from it. He has however been led to change his opinion concerning the cause of contraction, as will appear further on.

not become solid at the same time, we may admit that when the outer crust became solid it must have assumed very nearly the temperature of the atmosphere, owing to its heat being radiated away into space. It has been thought that as the cooling proceeded, the interior shrunk away from the crust, and the latter became wrinkled; and that it was by this means that the crumpling and contortions of the rocks were produced. The simile sometimes made use of in Geological text-books is that of the skin of a shrivelled apple. However produced, these wrinkles exist, and where they have not yet been denuded and levelled down, they now form mountain chains, in which the crumpling of the rocks is usually conspicuous[1]. In other cases the tracts so elevated have been planed down by meteoric and marine action, afterwards covered by horizontal deposits, and re-elevated with greater or less secondary crumpling, into the positions where we now see them. That enormous force has been concerned in this crumpling of the hardest rock is obvious. Let us estimate its amount upon the present hypothesis; premising that we make no assumption whatever regarding the condition of the interior of the globe nor the cause of contraction, but merely assume that it has shrunk away from a rigid upper crust. The problem is mathematically analogous to that of finding the tension of a flexible surface exposed to the pressure of a fluid.

[1] Newton, in a letter (1681) to Burnet, Master of the Charterhouse, speculates on the origin of mountains. He suggests an analogy between the coagulation of a "uniform chaos" into "heterogeneous veins or masses to cause hills," and the manner in which "an uniform solution of saltpetre coagulates into long bars," and "tin congeals in lumps when the fluider part of the tin, which congeals not so soon, is run from between them." He suggests that the "pressure of the moon or vortex etc. may promote the irregularity of the causes of hills," but does not "design to explain the generation of hills thereby." At the conclusion of a long letter we read, "I forbear to describe other causes of mountains, as the breaking out of vapours before the earth was well hardened—the settling and shrinking of the whole globe after the upper regions or surface began to be hard." If Newton by "shrinking" had meant shrinking through cooling, then the contraction theory had suggested itself to his fertile mind; but he seems rather to refer to a subsequent drying of the matter, which, "gravitating towards a centre, shrunk closer together, and at length, a great part of it condensing, subsided in the form of a muddy water or limus to compose this terraqueous globe." See Brewster's "Life," vol. II. App. VI. p. 447.

Suppose $PTQS$ to be a rectangular element of the crust, the four sides being equal and the thickness being τ, and let



PQ and ST be the curves of greatest and least curvature on its surface. These are by a theorem in geometry necessarily orthogonal. Let RO, RO', the corresponding radii of curvature, be called r, r'.

Let T be the pressure on unit of area of the section of the crust in the directions of the tangents at P and Q which is produced by the lateral thrust. Similarly let T' be the pressure in the direction of the tangents at S and T. The weight of the element is $g\rho PQ \times ST \times \tau$, and acts in the normal ROO'.

Let the angles $$POQ = \theta,$$ $$SO'T = \phi.$$

If then we suppose this element supported by a pressure R on unit of area from beneath, resolving the forces vertically we have for the equation of equilibrium

$$2Tr'\phi\tau \sin\frac{\theta}{2} + 2T'r\theta\tau \sin\frac{\phi}{2} = g\rho r\theta r'\phi\tau - Rr\theta r'\phi.$$

In the limit, dividing by $r\theta r'\phi$ this gives

$$\left(\frac{T}{r}+\frac{T'}{r'}\right)\tau = g\rho\tau - R.$$

If we regard the surface as spherical and of radius a, T and T' become equal, and we have

$$2\frac{T\tau}{a} = g\rho\tau - R.$$

If the support beneath is withdrawn altogether R becomes nothing and then

$$T = \tfrac{1}{2}g\rho a,$$

which gives the maximum thrust which would be produced by the shrinking away of the underlying mass.

The meaning of this result is that the horizontal force of compression thrown into a stratum at the earth's surface by the shrinking away of the underlying parts would be equal to the weight of a piece of the same stratum of the same section as the stratum and *two thousand* miles long—enough to crumple up and distort any rocks. It would be about 830,200 tons upon the square foot[1].

The pressures T and T' being orthogonal cannot affect one another. Suppose then T and r given. It follows that if r' is increased or diminished, T' must be increased or diminished in the same proportion. Hence, if the curvature be diminished the pressure will be proportionately increased and *vice versâ*. Hence there will be no greater tendency to compression where the curvature is greater, as about the equator, than where it is less, as about the poles, but the reverse. This answers an objection which has been sometimes brought against the theory; although apart from the above reasoning it is scarcely likely that so small a difference of curvature as that referred to can have any appreciable consequence.

It is inconceivable that such rock as we find at the surface of the earth could support so great a pressure as this without bending and breaking. Hence, if it be supposed possible that for a moment a shell of moderate thickness, say a few miles,

[1] Calculated from a cube of granite weighing 178·339 lbs.

should be self-supporting, and therefore exert no pressure upon that immediately beneath it, this state of things could not continue, and the outer shell would give way, and settle down upon the next stratum below it. We are not able however to estimate the downward pressure which will be exerted by it at any point upon the next lower stratum, because the lateral pressure T might not be wholly relieved, but only partially so, and so to some extent help to support the shell. It is evident that in such a case the downward pressure would not be quite the same everywhere, the tilted rocks in some places supporting each other, and pressing with diminished weight upon the subjacent matter.

If the rock were perfectly rigid T would have its full value, the shell would be self-supporting, and the vertical pressure on the next stratum would be nothing. If on the other hand the rock were fluid, such pressure would be that due to the depth τ of the upper layer. Its actual value will be something intermediate, but probably not differing much from that due to its mere depth.

The above estimate of the amount of the horizontal stress in a stratum at the surface of the earth assumes that the force of gravity is constant throughout its whole thickness. It will appear further on, that, for the present purpose, and for the greatest thickness of the crust which can have been subject to compression owing to collapse of the interior through cooling, this assumption is legitimate; and that we need not trouble ourselves at present about a change in the value of gravity[1].

We may admit then that the horizontal stress, which would be generated by contraction of the matter beneath the crust, would be abundantly powerful to produce that crushing together of the strata, which is so common a phenomenon among rocks of all ages. But whether the observed effect has been actually produced in that manner, can only be decided by further considerations.

(1) Would the contraction of the interior through cooling

[1] In the first edition a somewhat tedious calculation was given to find the compression of the more central shells. This is now omitted as superfluous.

have been sufficient in amount to give rise to chains of mountains of the magnitude of those which exist?

(2) Would the chains be arranged upon the surface of the globe in the manner that they are?

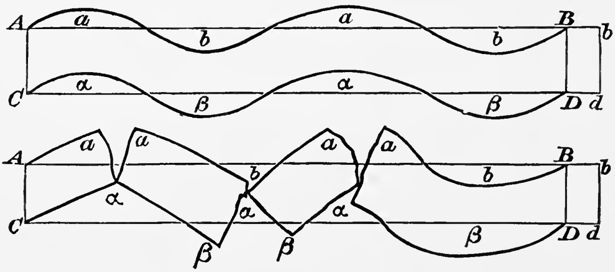
It is necessary to distinguish between the terms "compression" and "contraction." Contraction of the underlying rocks causes compression in the rocks above them. Of course contraction and compression may go on simultaneously in the same stratum. It may be compressed by the contraction of the rocks beneath it, and it may contract by itself cooling, or by losing some portion of its constituent substance. But for the present we shall consider that the stratum which is compressed does not at the same time contract, nor lose anything in volume by the compression to which it is subjected.

Geologists have commonly obtained their estimate of the amount of compression, to which a tract of country has been subjected, by supposing the folds flattened out, and then comparing the length of the strata so flattened with the actual length of the tract. But this method requires a knowledge of the entire length of each folded member, whereas we only see the upper part of each; and the synclinals are generally buried out of sight; and, moreover, estimates of the thickness of the strata which have been corrugated are liable to many sources of error from overlooking reduplications, attenuation by pressure, and other causes. An intensely corrugated stratum, in which the folds of the same bed are frequently repeated, may really represent less compression than one in which the dips are smaller, and the repetitions less frequent. Consequently this method is unsatisfactory, even when applied to limited areas. But it would be perfectly inapplicable to the surface of the entire globe.

Our first object then will be to find a relation between the quantity of the matter elevated and the amount of compression to which its elevation has been due.

Let $ABCD$ be a layer of rock originally flat, being then of length $l(1+c)$ and thickness k, and suppose the abutments at AC and BD to approach each other through the space lc where c is a small fraction, which we may call the coefficient of com-

pression. Then the layer of rock in question would assume some new form, becoming thickened, and contorted; suppose one of those given in the figure, or any other whatsoever conceivable[1].



Let us now seek for some simple laws which must govern the disturbed strata in spite of the confusion which appears to reign among them. Let a, a, &c., be the areas formed by the upper curved line above AB, and b, b, &c., the areas formed by the same line below AB. It is not necessary that the a's should be equal to one another, nor yet the b's. They are used simply to designate the areas in respect of their *positions*. We will call AB "the datum level."

In like manner let α, β, be similar areas for the lower datum level CD. Then the space included between the curved lines must be equal to $\quad ACdb = kl(1+c)$.

It is also evidently equal to

$$ACDB + a + a + \&c. + \beta + \beta + \&c.$$
$$- b - b - \&c. - \alpha - \alpha - \&c.,$$

or, denoting the sums of the quantities in like positions by the symbol Σ, we get

$$kl(1+c) = kl + \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha).$$

$$\therefore klc = \Sigma(a) - \Sigma(b) + \Sigma(\beta) - \Sigma(\alpha) \ldots\ldots\ldots\ldots (1),$$

which we shall call a "datum-level equation."

[1] Some interesting experiments have been made by Prof. A. Favre of Geneva upon the effect of compression in raising elevations of a plastic substance. He placed a layer of clay upon a stretched band of Caoutchouc, and then allowed it to contract. A description of the experiments, and figures illustrating some of the results, which closely resemble the contortions sometimes seen in the earth's strata, will be found in "Nature," vol. XIX. p. 103. See also Ch. XIII. seq.

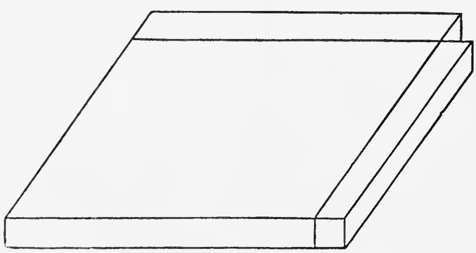
Since the pressure is supposed to take place in a horizontal direction, it will not have any direct effect to raise the centre of gravity of the portion of the crust under consideration; so that, if the layer in question rest upon a yielding substratum, so that some portions of the disturbed crust dip downwards, if the datum level be unaffected in position, a corresponding volume of the subjacent yielding rock must rise into the anticlinals.

Hence, $$\Sigma(\alpha) = \Sigma(\beta).$$

And the equation becomes

$$klc = \Sigma(a) - \Sigma(b).$$

In order to render this reasoning applicable to the section of a surface of any form it is only necessary that the pressure, which causes the compression, should be everywhere tangential to the surface, and that gravity should be perpendicular to it. Hence it is applicable to any layer of the crust, although its surface may not be strictly regular, and may contain local elevations and depressions affecting the *mean* figure (that is the figure as unaffected by corrugation), which, though of small amount as compared to the dimensions of the earth, may be large as compared with the quantities of which we have to take cognizance in this investigation.



In the next place it will be necessary to extend our considerations from a section of unit of horizontal width to an area corresponding to any proposed portion of the earth's surface, and eventually to that of the whole globe.

The diagram represents a portion of a layer of the crust

whose length is l and width w, and the thickness k; c and c' being the mean coefficients of compression in the directions of length and width. Then, if we neglect the product cc', our equation will become

$$klw\,(c+c') = \Sigma\,(A) - \Sigma\,(B),$$

where A and B are now the *volumes* of the elevations above and depressions below the datum level, which corresponds to the layer.

If in this equation we put $w=1$ and $c'=0$, it reduces to our former equation.

If we put $c=c'$ we get

$$2klwc = \Sigma\,(A) - \Sigma\,(B) \quad\text{.................}(2),$$

which we may take as the general expression corresponding to any area of the layer in question.

If the layer whose thickness is k rests upon a solid foundation, when it is compressed horizontally the corrugations can take place only in an upward direction, the solidity resisting their intrusion into the parts beneath. Hence we may introduce the supposition that $\Sigma\,(B)=0$, and the "datum-level equation" becomes

$$2klwc = \Sigma\,(A).$$

This result may be extended to an entire spherical layer of radius z by putting $lw = 4\pi z^2$, and it then becomes

$$8\pi z^2 kc = \Sigma\,(A).$$

What has been said refers to a layer of thickness k, throughout which the compression c is uniform, and which is all at the distance z from the centre of the globe. This is only true when the layer is taken of infinitesimal thickness. If we fix our attention upon such a layer, it is overlain by an infinity of other similar layers, each of which, on being corrugated, will push up those above it, and the resulting elevations, which will dominate the datum level at the surface, will be the integral effect of all the elevations contributed by the successive layers corrugated.

A clear conception of what will then be the upper "datum level" of the earth's surface is important. It will be an

imaginary surface which occupies the position that the surface of the crust would occupy at the present time, had it been perfectly compressible in a horizontal direction; so that no corrugations would have been formed in it; and similarly for the lower "datum level."

This definition of the datum levels involves the truth of the assertion already made, that, when a portion of the disturbed crust dips below the lower datum level, an equal volume of the substratum must rise into the anticlinals. For suppose the crust to be perfectly compressible. Then the enclosed matter will assume the form of a smooth sphere, whose surface is the lower datum level. Now let the crust expand so as to become corrugated. The enclosed matter being incompressible and unaltered in volume, whatever quantity of it is displaced below the lower datum level must find accommodation above it; hence as stated we must have,

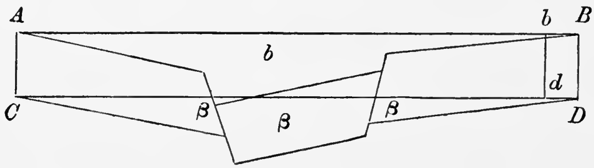
$$\Sigma(\alpha) = \Sigma(\beta).$$

It will be noticed that the datum levels as above defined will have their positions fixed simply by the amount of contraction of the interior.

As soon as elevated tracts had once been formed by the corrugation of the cooled crust, all the material above the datum level, however distributed, must have been derived from matter originally beneath it, and subsequently raised above it. As often as further compression of the crust has taken place, every fresh addition to the sum total of the quantity of matter raised above the datum level must have accrued by the elevation of matter from below it. For the matter which was already above it, however freshly corrugated, or rearranged by water action, or otherwise, cannot have been thereby altered in quantity. Moreover each additional contraction will have acted upon a crust thicker than it was before, on account of its having become in the meanwhile solid, or if solid contracted, to a greater depth, and $\Sigma(A)$ being then the total volume of the elevations at the surface where the radius is r, $\frac{\Sigma(A)}{4\pi r^2}$ will be their mean height, or the thickness (h) of the layer which they

would form over the whole surface if they were levelled down; so that

$$h = \frac{\Sigma(A)}{4\pi r^2}.$$



We can readily adapt the datum-level equation to the case of horizontal contraction (as distinguished from compression): for suppose the layer $ABCD$ to be stretched between the fixed abutments AC and BD, owing to the substance of which it is composed contracting by a length bB or dD equal to cAB, or cl, the contraction being distributed anyhow between AC and BD.

Then the space included between the broken lines must be equal to $AbdC$, or to $kl(1-c)$.

Hence $$kl = kl(1-c) + \Sigma(b) - \Sigma(\beta).$$

$$\therefore klc = \Sigma(b) - \Sigma(\beta),$$

which is of the same form as before, except that $\Sigma(b)$ and $\Sigma(\beta)$ have now changed signs. In this case it appears that $\Sigma(a)$ and $\Sigma(\alpha)$ are necessarily absent, because horizontal contraction cannot cause elevation.

$\Sigma(b)$ and $\Sigma(\beta)$ are the depressions below the upper and lower datum levels; those levels being defined by supposing the layer in question to be perfectly extensible, or, what amounts to the same thing, not to contract horizontally under the influence tending to cause contraction. It must not however be forgotten, that the distance between the datum levels will become diminished in the ratio of the vertical contraction.

If the layer rests upon a solid foundation $\Sigma(\beta) = 0$, and then, as in the second figure,

$$klc = \Sigma(b).$$

And if, as in the case of compression, we call $\Sigma(B)$ the volume of the depressions, and w the width of the tract under consideration,

$$2klwc = \Sigma(B).$$

And for a spherical shell at a distance z from the centre of the sphere,

$$8\pi z^2 kc = \Sigma(B).$$

Hence, if η be the mean depth below the datum level of all the depressions at the surface,

$$\eta = \frac{\Sigma(B)}{4\pi r^2} = 2\pi kc.$$

A simple mode of regarding the subject is to consider $\Sigma(B)$ as the total volume of all the chasms which would be produced by horizontal contraction. These are afterwards filled up by faulting or other movements of settlement, producing possibly an irregular outline of the upper surface. It is then theoretically conceivable, although practically most improbable, that, here and there, there may be points of the resulting surface which remain undisturbed, like A and B in the diagram. These points would then be upon the datum level, and in that case η would be the mean depth of the volume of the depressions below these culminating points. But, in fact, the actual mean depth below the highest points would be certainly less than η, and might be anything between η and nothing. In short η is the superior limit of the possible mean of the total depressions below the existing culminating points.

CHAPTER VIII.

RESULTS OF THE COOLING OF THE EARTH SUPPOSING IT SOLID THROUGHOUT[1].

Results of cooling in three cases—(1) suddenly by the same amount—(2) gradually without settling together—(3) gradually with settling together—Level of no strain of Mr Reade and Mr Davison—Calculation of its depth—Approximately this does not depend upon the coefficient of contraction—This depth is proportional to the whole time of cooling—Calculation of an expression for the mean height of elevations—Selection of a coefficient of contraction—Numerical results for depth of level of no strain, for its temperature, for total radial contraction, for depth of level of greatest cooling, and for depth of level of greatest stretching—Superior limit of depth of depressions that could be formed—Depth and temperature of level of greatest stretching.

In the preceding Chapter we have proposed two questions; the first of which is, would the contraction of the interior of the earth through cooling have been sufficient in amount to give rise to chains of mountains of the magnitude of those which exist? To answer this enquiry our first step shall be to calculate the amount of elevations, which would result from the cooling of the earth on the supposition that it is solid throughout; and the whole subject will be made more clear by previously considering what would be likely to happen in two purely hypothetical cases.

1. If a hot sphere were to be cooled suddenly throughout by the same amount, the linear contraction being in that case the same in all directions, it would become simply smaller, without any tendency to either cracking or crumpling.

2. If, however, the outer strata were to cool gradually and

[1] This Chapter contains the substance of papers communicated by the Author to the "Philosophical Magazine" in Nov. 1887, and Jan. 1888.

more than the inner ones, it is clear that they would become too small to fit the uncooled nucleus; and this effect would reach down to the level at which cooling, and therefore contraction, became insensible. Supposing, then, that nothing further happened to the rocks than a simple contraction, the strata must needs crack; and we may imagine that the crust would be divided up by fissures, widely gaping towards the surface, into prisms similar in form to basaltic columns, and reaching down to the uncooled matter. The vertical thickness of a crust so cooled would be diminished by the sum of the linear contractions of the thicknesses of each infinitesimally thin shell in accordance with the law of simple contraction, and the circumference of each shell, not counting the width of the cracks, would be shortened in proportion to the entire fall of temperature which had been experienced by that shell. The result in this second case, depending solely upon the fall of temperature, would be independent of the time.

3. Turning next to a third case, more nearly approaching what might be supposed to occur to a solid earth during the fall from the uniform high temperature of solidification to that which is its present distribution, the rate of cooling would be different at different depths. At any epoch, since the surface assumed the constant temperature of the atmosphere, the cooling at the surface is *nil*. At a certain depth, where the cooling is insensible, it is again practically *nil*. At some intermediate depth, depending on the time, the rate of cooling is greatest; and where it is greatest, there the rate of contraction will be greater than anywhere above or below that depth. In the case we are considering it is not probable that open cracks could anywhere be formed, unless just near the surface, because the weight of the superincumbent matter would press out the contracting shells laterally, so as to close them up. Under these circumstances we could not in general arrive at the change of dimensions by applying the coefficient of linear contraction to the horizontal and vertical dimensions separately of each shell; but wherever the shell is extended (or "stretched") we can merely apply the coefficient of voluminal contraction to the shell as a whole.

Let us now fix our attention upon the condition of a particular spherical shell of rock at the present epoch. We find it continuous and without open cracks, its temperature is falling, and, owing to the contraction of the sphere of matter interior to it, the shell is about to sink into a position where, being nearer to the centre of the sphere, it will find less room to occupy. The question then is—Will the horizontal linear contraction of this shell exceed or fall short of that loss of room? If it exceeds, the shell will tend to be extended. If, on the other hand, the contraction is less than the loss of room due to the sinking through shortening of the radius, the shell will be compressed. Mr T. Mellard Reade[1] and Mr Davison[2] have shown that near the surface the shells are being compressed, and deeper down extended, and that there is a certain level of no strain, where there is neither extension nor compression, and that this level sinks deeper as time goes on. We see, then, that no compression has ever taken place below the level of no strain, and that, between the surface and it, all the shells have successively passed from a state of extension into one of compression. In calculating the amount of compression in this third case, it will be necessary to have regard to the position of the level of no strain at every successive moment from the commencement of the cooling, because it defines at that moment the limit, below which compression does not reach. Any calculation, in which we did not integrate for the time, would therefore give an incorrect result in the case we are considering.

We now follow the third hypothesis, viz., that the earth has cooled as a solid, and that there has always been within the crust a level of no strain, below which the elementary spherical shells have tended to be extended, and above it to be compressed; and we make the probable (though not certain) hypothesis that, below the level of no strain, during the process of contraction, the interior sphere remains without vacancies—that is, the substance settles together by what Mr Reade calls "compressive extension" and Mr Davison "stretching." This

[1] "The Origin of Mountain Ranges," London, 1886, chap. XI.

[2] "Phil. Trans. Roy. Soc." vol. 178, 1887, pp. 231—249.

precludes our applying separately the coefficient of linear contraction to the vertical and horizontal dimensions throughout that portion; but we may apply to it the voluminal coefficient only, which is *generally* applicable.

Retaining the symbols used by Sir William Thomson in his paper on secular cooling[1], let

r = the radius of the earth at present, taken at 20,900,800 feet,

t = the time elapsed since the globe solidified throughout,

V = the temperature of solidification,

x = the distance of a spherical shell of elementary thickness dx from the surface at the time t,

z = the distance of the same shell from the centre,

z' = the distance of the same at the time $t + dt$,

v = the temperature of the said shell at the time t,

θ = the fall of temperature of the shell,

ϕ = the fall of temperature of the level of no strain,

E = the coefficient of voluminal contraction,

e = the coefficient of linear contraction,

h = the mean height of the surface-elevations formed.

Two other quantities are involved, which are defined in Sir W. Thomson's paper, wherein he shows that, at the depth x, at the time t,

$$v = C + \frac{b}{a}\int_0^x \epsilon^{-\frac{x^2}{a^2}} dx,$$

where b is a temperature such that

$$b = \frac{V}{\frac{1}{2}\sqrt{\pi}},$$

and a is a length such that

$$a = 2\sqrt{\kappa t},$$

κ being the conductivity of the substance expressed in terms of its own capacity for heat[2].

[1] "Trans. Roy. Soc. Edinb." vol. XXIII. part 1, p. 157; also "Phil. Mag." [4] vol. XXV.; and Thomson and Tait's "Natural Philosophy," Appendix D.

[2] See p. 66.

To find the Depth of the Level of no strain.

The volume of the shell at the depth x is $4\pi(r-x)^2\,dx$, and it will be changed in the interval dt by

$$E4\pi(r-x)^2 dx \frac{dv}{dt} dt.$$

The whole change in volume of the sphere interior to this shell will therefore be

$$E4\pi \int_x^r (r-x)^2 \frac{dv}{dt}\, dt\, dx.$$

So that the volume of the sphere interior to this shell will become

$$\frac{4}{3}\pi(r-x)^3 + \frac{3}{3} E4\pi \int_x^r (r-x)^2 \frac{dv}{dt}\, dt\, dx\,;$$

and, neglecting E^2, its radius will be

$$(r-x) + \frac{E}{(r-x)^2}\int_x^r (r-x)^2 \frac{dv}{dt}\, dt\, dx.$$

The circumference of the interior sphere will therefore be altered in the interval dt by

$$\frac{2\pi E}{(r-x)^2}\int_x^r (r-x)^2 \frac{dv}{dt}\, dt\, dx.$$

It is evident that, if the diminution of this circumference is equal to the horizontal contraction of the shell next above it, that shell will neither be compressed nor extended. But the horizontal contraction of that shell will be

$$-2\pi e(r-x)\frac{dv}{dt}\, dt\, dx.$$

Hence the condition that the shell at x is situated at the level of no strain will be, since $E = 3e$,

$$\frac{3e}{(r-x)^2}\int_x^r (r-x)^2 \frac{dv}{dt}\, dx = e(r-x)\frac{dv}{dt}.$$

It will be observed that the position of this level of no strain does not depend on the coefficient of contraction, which will divide out.

According as

$$\frac{3e}{(r-x)^2}\int_x^r (r-x)^2 \frac{dv}{dt}\,dx \gtreqless e\,(r-x)\frac{dv}{dt},$$

so will the shell be compressed, not strained, or extended.

Now the fundamental partial differential equation for the conduction of heat gives

$$\frac{dv}{dt} = \kappa\frac{d^2v}{dx^2}.$$

Making this substitution, the above becomes

$$\frac{3e}{(r-x)^2}\int_x^r (r-x)^2 \frac{d^2v}{dx^2}\,dx \gtreqless e\,(r-x)\frac{d^2v}{dx^2}.$$

If we integrate by parts, so as to raise the index of $(r-x)^2$, observing that $\frac{d^2v}{dx^2}$ may be put $=0$ when $x=r$, we can obtain Mr Davison's expression No. 2 in the 'Proceedings' of the Royal Society[1].

Putting the members of the above as an equality, the value of x will give the depth of the level of no strain.

If we integrate $\frac{d^2v}{dx^2}$ first, we get by parts

$$\int_x^r (r-x)^2 \frac{d^2v}{dx^2}\,dx = -(r-x)^2\frac{dv}{dx} + \int_x^r 2\,(r-x)\frac{dv}{dx}\,dx$$

$$= -(r-x)^2 \frac{V}{\sqrt{\pi\kappa t}}\,\epsilon^{-\frac{x^2}{4\kappa t}} + \int_x^r 2\,(r-x)\frac{V}{\sqrt{\pi\kappa t}}\,\epsilon^{-\frac{x^2}{4\kappa t}}\,dx.$$

But

$$\frac{d^2v}{dx^2} = \frac{V}{\sqrt{\pi\kappa t}}\,\epsilon^{-\frac{x^2}{4\kappa t}}\left(-\frac{2x}{4\kappa t}\right).$$

Hence, dividing by $\frac{-V}{\sqrt{\pi\kappa t}}$, transposing, and writing a^2 for $4\kappa t$, we have for the equation to find x,

$$\frac{3}{(r-x)^2}\left\{(r-x)^2\,\epsilon^{-\frac{x^2}{a^2}} - \int_x^r 2\,(r-x)\,\epsilon^{-\frac{x^2}{a^2}}\,dx\right\} = (r-x)\,\epsilon^{-\frac{x^2}{a^2}}\frac{2x}{a^2}.$$

[1] Vol. XLIII. p. 326.

Multiply by $\frac{(r-x)^2}{6r}$, and transpose.

$$\frac{(r-x)^2}{2r}\epsilon^{-\frac{x^2}{a^2}} - \frac{a^2}{2r}\int_x^r\left(-\frac{2x}{a^2}\epsilon^{-\frac{x^2}{a^2}}\right)dx - \frac{(r-x)^3}{6r}\epsilon^{-\frac{x^2}{a^2}}\frac{2x}{a^2} = \int_x^r \epsilon^{-\frac{x^2}{a^2}}dx.$$

The term to be integrated on the left hand side becomes

$$-\frac{a^2}{2r}\left(\epsilon^{-\frac{r^2}{a^2}} - \epsilon^{-\frac{x^2}{a^2}}\right)$$

when taken between the limits.

Let us take a for the unit of length. Then ϵ^{-r^2} being extremely small may be neglected; and the equation may be written,

$$\frac{1}{2r\epsilon^{x^2}}\left\{(r-x)^2 + 1 - \frac{2}{3}(r-x)^3 x\right\} = \int_x^r \epsilon^{-x^2}dx \ldots\ldots (A).$$

The value of x given by this equation will be the depth of the level of no strain in terms of a, or $2\sqrt{\kappa t}$, as the unit of length.

The large negative term in $(r-x)^3 x$ warns us that x cannot be otherwise than very small.

Since $$\int_x^r \epsilon^{-x^2}dx = \int_0^r \epsilon^{-x^2}dx - \int_0^x \epsilon^{-x^2}dx,$$

and since $$\int_0^r \epsilon^{-x^2}dx = \frac{1}{2}\sqrt{\pi} = 0\cdot8862$$

because r is large, if we expand the terms of the above equation neglecting powers of x above the square and of $1/r$ above the cube we obtain by solving the quadratic

$$x = \frac{3}{r^2}\left(\frac{r}{2} - \frac{1}{2}\sqrt{\pi} + \frac{1}{2r}\right).$$

And on restoring the unit a, this gives for the depth of the level of no strain

$$x = \frac{3}{2}\left(\frac{a^2}{r} - \sqrt{\pi}\frac{a^3}{r^2} + \frac{a^4}{r^3}\right).$$

If we write for a^2 its value $4\kappa t$, and call the depth of the level of no strain x_0, we have,

$$x_0 = \frac{3}{2}\left(\frac{4\kappa t}{r} - \sqrt{\pi}\frac{(4\kappa t)^{\frac{3}{2}}}{r^2} + \frac{(4\kappa t)^2}{r^3}\right) \ldots\ldots\ldots\ldots (B).$$

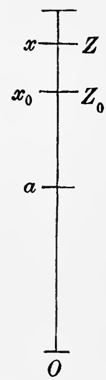
At the present time, on the supposition that the temperature of solidification was 7000° F., a would be 402832 feet, and then

the depth of the level of no strain would be 11252 feet, or 2·1311 miles.

As a first approximation $x_0 = \frac{6\kappa t}{r}$, which is Professor Darwin's value, and varies as the time since solidification[1]. For larger values of the time the second term would need to be taken in, so that $x_0 \propto mt - nt^{\frac{3}{2}}$. But the first term will suffice for our problem.

We now proceed to find the mean height of the elevations above the datum level, which would result from the compression of the matter above the level of no strain.

Let z_0 be the value of z, and x_0 of x, at the level of no strain at any time t. Then at that time the shells above Z_0 are being compressed, and the coefficient of linear contraction may be applied to them in the horizontal and vertical dimensions separately. But we cannot so apply the coefficient to the shells below Z_0, where the shells are undergoing what Mr Reade aptly calls "compressive extension."



From what has already been proved, it appears that the position of the level of no strain does not depend upon the numerical value of the coefficient of contraction; and that, if we neglect the second term in (B),

$$z_0 = r - \frac{3}{2}\frac{4\kappa t}{r} = r - mt, \text{ suppose,}$$

where m is very small, being $\frac{24}{209008}$.

$\frac{d\phi}{dt}$ is the rate at which at the time t the temperature of the shell at the level of no strain is falling[2]. Let Z be the position of a shell under compression between Z_0 and the surface, $\frac{d\theta}{dt}$ the rate at which the temperature of Z is falling, O the centre of the sphere.

[1] See "Phil. Trans. Roy. Soc.," vol. 178, p. 247, 1887. Mr Davison on the other hand states that the depth varies as the square root of the time, but gives no formula for the value of it. *Ibid.* p. 237.

[2] This definition is more correct than that of ϕ on p. 94.

Now the property of the level of no strain gives that the mean contraction of OZ_0 in the interval dt is the same as that of the circumference at Z_0, because, on contracting, the shell is neither stretched nor compressed.

Hence, owing to the change in position of that level in the time dt, OZ_0 is diminished by mdt; and, moreover, by the contraction of the radius according to the property just mentioned, it is also diminished by $e(r-mt)\frac{d\phi}{dt}dt$.

For like reasons, Z_0Z is increased by mdt and diminished by $\int_{z_0}^{z} e\frac{d\theta}{dt}dtdz$.

The mdt's cancel; and if we take z' as the value of z before the interval dt, we shall have

$$z' = z + e(r-mt)\frac{d\phi}{dt}dt + e\int_{z_0}^{z}\frac{d\theta}{dt}dtdz.$$

Now the contribution to the superficial elevations from the shell at Z, caused by compression during the interval dt arising from want of room, would be, if the shell did not contract horizontally, $(4\pi z'^2 - 4\pi z^2)\,dz$, where it is to be observed that z' contains dt.

But, owing to the horizontal contraction of the shell, this must be diminished by the areal contraction, or by

$$2e\frac{d\theta}{dt}dt\,4\pi z^2 dz.$$

Hence the contribution to the surface-elevations in the time dt from this shell will be

$$4\pi r^2\frac{d^2h}{dtdz}dtdz = (4\pi z'^2 - 4\pi z^2)\,dz - 2e\frac{d\theta}{dt}dt\,4\pi z^2dz.$$

$$\therefore\ r^2\frac{d^2h}{dtdz} = \frac{z'^2}{dt} - \frac{z^2}{dt} - 2ez^2\frac{d\theta}{dt}.$$

Substituting for z'^2, and neglecting terms in e^2,

$$r^2\frac{d^2h}{dtdz} = 2ez(r-mt)\frac{d\phi}{dt} + 2ez\int_{z_0}^{z}\frac{d\theta}{dt}dz - 2ez^2\frac{d\theta}{dt}.$$

Since $z = r - x$,

$$dz = -dx;$$

therefore $$\int_{z_0}^{z}dz = -\int_{x_0}^{x}dx = \int_{x}^{x_0}dx.$$

The equation then becomes

$$-r^2\frac{d^2h}{dtdx}=2e(r-x)(r-mt)\frac{d\phi}{dt}+2e(r-x)\int_x^{x_0}\frac{d\theta}{dt}dx-2e(r-x)^2\frac{d\theta}{dt}.$$

We are concerned only with values of x down to the level of no strain, where x is about 2 miles; so that the largest value of $\frac{x}{r}$ is about $\frac{2}{4000}$, and terms in $\frac{x}{r}$ may be neglected.

Now $$v = \text{const.} + \frac{b}{\sqrt{4\kappa t}}\int_0^x \epsilon^{-\frac{x^2}{a^2}}dx,$$

where $a^2 = 4\kappa t$.

Equation (A) p. 97 shows that when a, or $\sqrt{4\kappa t}$, is taken for the unit,

$$x_0 < \frac{1}{\frac{2}{3}(r-x)} + \frac{1}{\frac{2}{3}(r-x)^3};$$

therefore restoring a,

$$\frac{x_0}{a} < \frac{a}{\frac{2}{3}(r-x_0)} + \frac{a^3}{\frac{2}{3}(r-x_0)^3}.$$

In the present inquiry the order of increasing magnitude is x, x_0, a, and these all vanish when $t = 0$. So the above inequality shows that $\frac{x_0}{a}$ is zero when $t = 0$. *A fortiori* then is the ratio of $\frac{x}{a}$, which is always less than that of $\frac{x_0}{a}$, a quantity of the order of $\frac{a}{r}$, and therefore, when compared with its first power, its square may be neglected. This will reduce the equation of the temperature-curve to

$$v = \text{const.} + \frac{b}{\sqrt{4\kappa t}}x;$$

which signifies that, to the depth of the level of no strain, we have simply a family of straight lines to deal with, all starting from the same point on the surface, and becoming more nearly vertical as the time increases. This consideration will facilitate the calculation.

The above expression for v shows that the fall of temperature of the shell at the depth x will be expressible by

$$\theta = A - \frac{b}{\sqrt{4\kappa t}}x.$$

And the rate of its fall of temperature therefore will be

$$\frac{1}{2}\frac{b}{\sqrt{4\kappa}}t^{-\frac{3}{2}}x.$$

Hence, substituting for x the value which it has at the depth of no strain, the rate of fall of temperature of the shell at that depth, be it where it may at the time t, will be

$$\frac{d\phi}{dt} = \frac{1}{2}\frac{b}{\sqrt{4\kappa}}t^{-\frac{3}{2}} \times \frac{3}{2}\frac{4\kappa t}{r},$$

$$= \frac{3}{4}\frac{b}{r}\sqrt{\frac{4\kappa}{t}}.$$

Hence $$\int \frac{mt}{r}\frac{d\phi}{dt}\,dt = \frac{3}{4}b\left(\frac{\sqrt{4\kappa t}}{r}\right)^3.$$

And, since the greatest value which $\sqrt{4\kappa t}$ can have is a, this may be neglected.

It is also evident that $\int_x^{x_0} \theta dx$, when divided by r^2, may be neglected. Therefore, dividing by r^2, integrating for t, and neglecting the small quantities referred to, the equation is reduced to

$$-\frac{dh}{dx} = 2e\phi - 2e\theta$$

$$= 2e\left(\frac{3}{2}\frac{b}{r}\sqrt{4\kappa t} - A + \frac{b}{\sqrt{4\kappa t}}x\right) + f(x).$$

At the level of no strain $\frac{dh}{dx} = 0$, and $\sqrt{4\kappa t} = \sqrt{\frac{2rx}{3}}$,

whence $$f(x) = 2e\left(A - b\sqrt{\frac{6x}{r}}\right).$$

Substituting this value, and giving $\sqrt{4\kappa t}$ the value which it has at the present epoch, viz. a, we obtain,

$$-\frac{dh}{dx} = 2eb\left(\frac{3}{2}\frac{a}{r} + \frac{x}{a} - \sqrt{\frac{6x}{r}}\right).$$

$$\therefore \quad -h = 2e\frac{b}{a}\left(\frac{3}{2}\frac{a^2x}{r} + \frac{x^2}{2} - 2\sqrt{\frac{2}{3}}\frac{ax^{\frac{3}{2}}}{\sqrt{r}}\right) + C.$$

To obtain the mean height of the total elevations which can be formed out of all the portion of the crust which has been

subjected to compression up to the present time, the integral must be taken from $\frac{3}{2}\frac{a^2}{r}$ to 0.

Therefore, finally, the mean height of the total elevations is given by

$$h = 2e\frac{b}{a}\left\{\left(\frac{3}{2}\frac{a^2}{r}\right)^2 + \frac{1}{2}\left(\frac{3}{2}\frac{a^2}{r}\right)^2 - 3\left(\frac{a^2}{r}\right)^2\right\},$$

$$= \frac{3}{4}e\frac{b}{a}\left(\frac{a^2}{r}\right)^2.$$

In order to fix upon a value for e, the coefficient of contraction for 1° F., we may reduce the results of some experiments by Mallet upon large masses of slag run from the furnace[1]. They give $e = 0{\cdot}0000071$. The mean of six results obtained for the contraction of rocks at much lower temperatures by Mr Adie was 0·0000057[2], and Mr T. Mellard Reade's experiments lead to a like result[3]. We may therefore accept 0·0000071 with tolerable confidence as being at any rate large enough. $\frac{b}{a}$ is the temperature-gradient at present, which may be taken at $\frac{1}{51}$° F. per foot. The value of a, corresponding to 7000° F. as

[1] "Phil. Trans. Roy. Soc.," vol. 163, p. 201. See also "Nature," vol. XXII., p. 266.

To obtain the coefficient of contraction for 1° F. from Mr Mallet's experiments:

The cones of slag began to solidify when the containing iron moulds were at 450° F. At that time the mean contents of the moulds was 8231·9229 cubic inches, and the volume of the slag when measured at 53° F. was 7700·2303 cubic inches.

The slag entered the cones at a temperature of 3680°.

By Mr Mallet's estimate, founded on the *time* in which solidification commenced, the solidification commenced at about "3062°, or say 3000° F."

Hence the number of degrees through which the temperature fell was

$$3062° - 53° = 3009°.$$

We have therefore to determine E, the coefficient of cubical contraction between incipient consolidation and 53° F.

$$7700 = 8232\,(1 - E3009);$$

$$\therefore E = {\cdot}0000215,$$

$$\text{and } e = \frac{E}{3} = {\cdot}0000071.$$

[2] "Trans. Roy. Soc. Edin.," vol. XIII., p. 370. See also "Geol. Mag.," vol. x., p. 260.

[3] "Origin of Mountain Ranges," p. 112.

the temperature of solidification, is 402832 feet, and r, the radius, is 20900800 feet. With these numbers our result gives, for the mean height of all the elevations which would be formed upon a solid earth by cooling, from the beginning to the present time,

$$6\tfrac{1}{3} \text{ feet.}$$

Another very important fact is that the temperature of the level of no strain exceeds the surface-temperature by $b\frac{3}{2}\frac{a}{r}$ degrees only, which, with the high estimate of 7000° for solidification, gives an excess of 228° at present; so that no rock can ever have been pressed up from a depth where its temperature has been much higher than that of boiling water. Neither can it be replied that it may have been otherwise in former ages, because the above expression for the excess varies as the square root of the time and must have been less hitherto than it is now.

The property of the level of no strain enables us to calculate the contraction of the radius of a solid globe, cooling according to the supposed law; because the mean contraction from the centre to that level is the same as that of the circumference there. Therefore the mean contraction of z_0 in the interval dt is

$$ez_0 \frac{d\phi}{dt}\, dt \text{ which} = ez_0 \frac{3}{2}\frac{b}{r}\sqrt{\frac{\kappa}{t}}\, dt;$$

therefore in the whole time t it will be

$$\int_0^t e\,(r - mt)\frac{3}{2}\frac{b}{r}\sqrt{\frac{\kappa}{t}}\, dt,$$

or
$$e\frac{3}{2}\frac{b}{r}\sqrt{\kappa}\,(2rt^{\frac{1}{2}} - \tfrac{2}{3}mt^{\frac{3}{2}}).$$

The term in $\frac{m}{r}$ is negligible.

Hence the contraction from the centre to the level of no strain is

$$e\frac{3}{2}\,b\sqrt{4\kappa t} = e\frac{3}{2}\frac{b}{a}\,a^2;$$

which, with the assumed values, gives

$$6{\cdot}3272 \text{ miles.}$$

The result does not depend upon r, further than that it has been calculated upon the assumption that r is large compared with a.

The vertical contraction of the crust above this level is inconsiderable. It may be thus found:—

$$e\iint\frac{d\theta}{dt}\,dtdx = e\iint\frac{1}{2}\frac{b}{\sqrt{4\kappa}}\,t^{-\frac{3}{2}}x\,dtdx$$

$$= e\int\frac{1}{2}\frac{b}{\sqrt{4\kappa}}\,t^{-\frac{3}{2}}\frac{x^2}{2}\,dt,$$

which, being taken from $x=0$ to $x=mt$, the depth of the level of no strain,

$$= e\frac{1}{2}\frac{bm^2}{\sqrt{4\kappa}}\frac{2}{3}\frac{t^{\frac{3}{2}}}{2}$$

$$= \frac{e}{2}\frac{b}{a}\frac{3}{4}\frac{a^4}{r^2} = \frac{1}{2}h = 3 \text{ feet.}$$

We may therefore say that the

Radial contraction of the globe $= 6{\cdot}3$ miles.

The depth of the level where the rate of cooling is greatest may be found by equating to zero the differential coefficient of $\frac{dv}{dt}$ taken with regard to x, that is, if $\frac{d^2v}{dtdx} = 0$, then

$$x\epsilon^{-\frac{x^2}{a^2}}\left(-\frac{2x}{a^2}\right) + \epsilon^{-\frac{x^2}{a^2}} = 0.$$

$$\therefore x = \frac{a}{\sqrt{2}}.$$ [1]

The average compression of a layer of the crust at the depth x will be $\frac{dh}{dx}$; and consequently by putting $x=0$ in the expression for this quantity we find:

$$\text{Compression at the surface} = 2e\frac{b}{a}\frac{3}{2}\frac{a^2}{r},$$

$= 2e\frac{1}{51} \times$ depth of level of no strain expressed in feet.

This will be about 0·3 per cent. if the temperature of solidification was 7000° F., and 0·1 per cent. if it was 4000° F.

[1] See Prof. Darwin's letter to "Nature," Feb. 6, 1879, vol. XIX., p. 313.

It will be noticed that, in the above calculations, no reference has been made to the numerical value of the conductivity, nor to the time elapsed since the surface first assumed its present temperature. All that has been assumed is that the present temperature-gradient, expressed by b/a, is $1/51$, which is about the mean result of innumerable observations; and that the temperature of solidification was 7000° F. These have been sufficient for our purpose. However, the temperature-gradient being known, and the temperature of solidification assumed, there follows a relation between the time and conductivity; and to find this appears to have been the original object of Sir William Thomson's investigation[1]. If we make use of the value for the conductivity which he has deduced from certain observations on rocks *in situ* (viz. 400), the results just obtained will correspond to 98 millions of years. But a lower temperature, say 4000° F., seems more probable; in which case the time would be about 33 millions of years[2]. In that case all our estimates will have to be reduced. These depend upon the constant a; and

$$a = \frac{a}{b}\frac{2V}{\sqrt{\pi}} = 51\,\frac{2V}{\sqrt{\pi}}.$$

$$\therefore a \propto V.$$

If, then, we reduce the temperature from 7000° to 4000°, a will be reduced from 402832 feet to 230189 feet, and we shall have

$$h' = (\tfrac{4}{7})^4 \times h \text{ feet} = 8 \text{ inches}.$$

The excess of the temperature of the level of no strain over the surface-temperature $\propto V^2$; so that, if we put the surface-temperature at 50°, it is at present 124° F.; and, since it varies as $\sqrt{t}$, low as this value may seem, it is higher now than it has ever been.

The depth in feet of the level of no strain is $\frac{a}{b}$ into the excess of temperature, or $51 \times 74{\cdot}45$ feet.

Hence the depth of the level of no strain is now 3797 feet, or about 0·7 of a mile.

The radial contraction $\propto V^2$, and will therefore be 2 miles.

[1] See p. 64. [2] See p. 122, note.

Lastly,

since $$h = eb\frac{3}{4}\frac{a^3}{r^2},$$

$$h \propto t^{\frac{3}{2}}, \text{ and } \frac{dh}{dt} \propto \sqrt{t}.$$

This shows that the rate at which mountains would be formed is proportional to the square root of the time, and ought therefore to be greater at present than at any previous period.

Collecting our results, we may say that, on the two suppositions made respecting the temperature of solidification, we should have, at the present time:—

Temperature of Solidification......	7000° F.	4000° F.
Depth of level of greatest cooling......	54 miles.	31 miles.
Depth of level of greatest stretching...	53·6 miles.	30·7 miles[1].
Depth of level of no strain..............	2 miles.	0·7 mile.
Temperature of level of no strain......	278° F.	124° F.
Mean height of elevations	$6\frac{1}{3}$ feet.	8 inches.
Total contraction of radius	6 miles.	2 miles.
Mean compression at surface...........	0·003	0·001

It has been assumed in the foregoing calculations, that the matter below the level of no strain settles together gradually by "compressive extension," so that the level of no strain produced will at any time be sensibly free from inequalities. This seems to be most probable under the conditions of pressure arising from the weight of the superincumbent matter. Nevertheless it may be desirable to calculate the mean depth of the depressions below the surface datum level, which might be formed upon still another, or 4th hypothesis[2]—viz. that the matter does not settle together, through the vacuities caused by horizontal contraction being obliterated at the expense of the vertical thickness, but that they are closed by faulting, or compensated by stretching greater in some places than in others, so that the level of no strain, if one is formed at all, will not be

[1] See p. 110. [2] See pp. 91, 92.

free from inequalities. In this case we may apply the principle of the datum level as suited to the case of horizontal contraction, and illustrated by the diagrams on page 89, bearing in mind the remark there made, that the result will give a maximum value, which the mean depth of the depressions cannot exceed, while they will probably be very much less.

Suppose the shells below Z_0 to contract so that the coefficient of contraction may be applied to the horizontal and vertical dimensions separately, the shortening in the horizontal direction being satisfied by faulting or by stretching; let z' be the distance of the shell at z from the centre at the time t.

Then
$$z' = z + e\int_0^z \frac{d\theta}{dt}\,dtdz.$$

When the shell which was at z' sinks to z, it will tend to be compressed, because it becomes the envelope of a smaller sphere. And the areal compression from this cause will be

$$4\pi z'^2 - 4\pi z^2.$$

At the same time it will tend to be extended on account of the areal contraction due to the cooling of the shell itself, and this contraction will be

$$4\pi z^2 2e\,\frac{d\theta}{dt}\,dt.$$

We have the contribution to the mean surface depression (η) from the shell at z of thickness dz in the interval dt

$$4\pi r^2\,\frac{d^2\eta}{dtdz}\,dtdz = 4\pi z^2 2e\,\frac{d\theta}{dt}\,dtdz - 4\pi 2ez\left(\int_0^z \frac{d\theta}{dt}\,dtdz\right)dz.$$

$$\therefore\ r^2\,\frac{d^2\eta}{dtdz} = 2ez^2\,\frac{d\theta}{dt} - 2ez\int_0^z \frac{d\theta}{dt}\,dz.$$

But
$$\theta = \text{const.} - v;\ \therefore\ \frac{d\theta}{dt} = -\frac{dv}{dt} = -\kappa\,\frac{d^2v}{dx^2}.$$

Also
$$z = r - x \ \text{ and } \ dz = -dx.$$

And
$$-\int_0^z dz = +\int_z^0 dz = -\int_x^r dx.$$

$$\therefore -r^2\frac{d^2\eta}{dtdx} = -2e(r-x)^2\kappa\frac{d^2v}{dx^2} + 2e(r-x)\int_x^r \kappa\frac{d^2v}{dx^2}dx.$$

But, $$\int_x^r \kappa\frac{d^2v}{dx^2}dx = -\kappa\frac{dv}{dx}, \text{ since } \frac{dv}{dx_{=r}} = 0.$$

Hence, changing all the signs,

$$r^2\frac{d^2\eta}{dtdx} = 2e(r-x)^2\kappa\frac{d^2v}{dx^2} + 2e(r-x)\kappa\frac{dv}{dx}.$$

Integrate for x.

$$r^2\frac{d\eta}{dt} = 2\kappa e\left\{(r-x)^2\frac{dv}{dx} + \int 2(r-x)\,dx\frac{dv}{dx} + \int (r-x)\frac{dv}{dx}dx\right\},$$

$$= 2\kappa e\left\{(r-x)^2\frac{dv}{dx} + 3\frac{b}{a}\int (r-x)\,\epsilon^{-\frac{x^2}{a^2}}dx\right\}.$$

But $$\frac{b}{a}\int (r-x)\,\epsilon^{-\frac{x^2}{a^2}}dx = \frac{b}{a}r\int \epsilon^{-\frac{x^2}{a^2}}dx + \frac{ab}{2}\epsilon^{-\frac{x^2}{a^2}},$$

$$= br\int \epsilon^{-\frac{x^2}{a^2}}d\left(\frac{x}{a}\right) + \frac{ab}{2}\epsilon^{-\frac{x^2}{a^2}}.$$

$$\therefore r^2\frac{d\eta}{dt} = 2\kappa e\left\{(r-x)^2\frac{dv}{dx} + 3br\int \epsilon^{-\frac{x^2}{a^2}}d\left(\frac{x}{a}\right) + \frac{3}{2}ab\epsilon^{-\frac{x^2}{a^2}}\right\}.$$

This measures the time-rate at which depressions at the surface are being formed from the spherical mass interior to the shell at x. No depression is contributed from the centre, where $x = r$, so that no constant need be added.

If we put for x the depth of the level of no strain, we shall have the time-rate for the whole of the depressions.

In that case x becomes x_0, and is at the most about 2 miles, and $\frac{dv}{dx}$ may be put $= \frac{b}{a}$.

Now $$\int_{\frac{r}{a}}^{\frac{x_0}{a}} = -\int_{\frac{x_0}{a}}^{\frac{r}{a}} = -\left(\int_0^{\frac{r}{a}} - \int_0^{\frac{x_0}{a}}\right),$$

$$\therefore \int_{\frac{r}{a}}^{\frac{x_0}{a}} \epsilon^{-\frac{x^2}{a^2}}d\left(\frac{x}{a}\right) = -\frac{1}{2}\sqrt{\pi} + \frac{x_0}{a} \text{ approximately},$$

so that $$r^2 \frac{d\eta}{dt} = 2\kappa e \left\{ (r - x_0)^2 \frac{b}{a} + 3br\left(\frac{x_0}{a} - \frac{1}{2}\sqrt{\pi}\right) + \frac{3}{2} ab \right\},$$

whence $$\frac{d\eta}{dt} = 2\kappa e \left\{ \left(1 - 2\frac{x_0}{r}\right)\frac{b}{a} + \frac{3b}{r}\left(\frac{x_0}{a} - \frac{1}{2}\sqrt{\pi}\right) + \frac{3}{2}\frac{ab}{r^2} \right\}.$$

Now $$x_0 = \frac{3}{2}\frac{a^2}{r}; \quad \therefore \frac{x_0}{a} = \frac{3}{2}\frac{a}{r}.$$

Making this substitution and reducing,

$$\frac{d\eta}{dt} = 2\kappa e \left\{ \frac{b}{a} + 3\frac{ab}{r^2} - \frac{3}{2}\frac{b}{r}\sqrt{\pi} \right\}$$

$$= 2\kappa eb \left\{ \frac{1}{\sqrt{4\kappa t}} + 3\frac{\sqrt{4\kappa t}}{r^2} - \frac{3}{2}\frac{1}{r}\sqrt{\pi} \right\}.$$

Integrating for t, since

$$\eta = 0 \text{ when } t = 0,$$

$$\therefore \eta = 2eb \left\{ \sqrt{\kappa t} + \frac{4(\kappa t)^{\frac{3}{2}}}{r^2} - \frac{3}{2}\frac{\sqrt{\pi}}{r}\kappa t \right\},$$

$$= eb \left\{ a + \frac{a^3}{r^2} - \frac{3}{4}\sqrt{\pi}\frac{a^2}{r} \right\},$$

$$= e\frac{b}{a} \left\{ a^2 + \frac{a^4}{r^2} - \frac{3}{4}\sqrt{\pi}\frac{a^3}{r} \right\}.$$

This gives

when $V = 7000°$ F., mean depth of depressions $= 4{\cdot}17$ miles;

when $V = 4000°$ F., mean depth of depressions $= 1{\cdot}37$ miles.

$\dfrac{d^2\eta}{dtdx}$ is the rate at which the shell at x contributes its share to the depth of the total depressions at the surface. Hence, to find what shell is contributing most rapidly at any given moment, we must find what value of x makes this a maximum, and must therefore equate to zero its differential coefficient with regard to x.

$$\therefore (r - x)^2 \frac{d^3v}{dx^3} - 2(r - x)\frac{d^2v}{dx^2} + (r - x)\frac{d^2v}{dx^2} - \frac{dv}{dx} = 0.$$

Putting for $\dfrac{dv}{dx}$ its value $\dfrac{b}{a}\epsilon^{-\frac{x^2}{a^2}}$ and similarly for the other differential coefficients we obtain from this, if we neglect $\dfrac{x^2}{r^2}$,

$$\frac{2x^2}{a^2} - \frac{4x^3}{ra^2} + \frac{3x}{r} = 1 + \frac{a^2}{2r^2}.$$

Neglecting all the terms with r in the denominator, this gives as a first approximation,

$$x = \frac{a}{\sqrt{2}}.$$

Substitute this value in the second term, and solving the quadratic, we obtain to the first power of $1/r$

$$x = \frac{a}{\sqrt{2}} - \frac{1}{4}\frac{a^2}{r}$$

as the depth at which the stretching is greatest.

We have already seen that the cooling is most rapid at the depth $\frac{a}{\sqrt{2}}$. Hence the stretching is greatest at a depth slightly less than that of greatest rate of cooling, the difference being

$$\frac{1}{4}\frac{a^2}{r}.$$

If the temperature of solidification is taken at 7000° F. the greatest stretching at the present epoch will occur at the depth of 53·463 miles—and if the temperature of solidification was 4000° F. it will be at present at the depth of 30·55 miles.

The temperature at this depth can be easily shown to be

$$b\int_0^{\frac{1}{\sqrt{2}}} \epsilon^{-z^2}\,dz \text{ nearly,}$$

which is equal to $0·68466V$ and is therefore the same for all times.

If, as is assumed, on the hypothesis of solidity, the temperature of solidification is raised by pressure, it seems nearly certain that the relaxation of pressure involved in stretching would induce fusion at the temperature of the level of greatest stretching, where the temperature is so nearly the melting temperature under pressure; and *a fortiori* it would do so at still greater depths where the temperature is higher. This would bring compressive extension into play, and greatly reduce the probability (in any case small) of localization of stretching under specialized areas.

CHAPTER IX.

THEORIES TO ACCOUNT FOR INEQUALITIES OF SURFACE.

The cause of inequalities on the earth's surface a prime object of inquiry—An actual measure of them sought—It may be expressed in terms of depth of sea and height of land and areas of the same—This measure estimated—And found much greater than can be accounted for by contraction—Reasons for believing the inequalities not pristine—Objections have been made to this belief—Prof. Darwin's theory of tidal wrinkles—Does not account for subsequent changes of level—Comparison with the form and distribution of continents—Mr Davison's theory of stretching beneath oceans—Difficulties in accepting this theory.

INASMUCH as one chief subject of our inquiry relates to the mode of formation of the inequalities of the earth's surface, it is important to seek for a fairly reliable measurement of their mean height; which may be done by estimating how thick a coating they would form over the whole globe if they were levelled down. This, when found, will serve as a standard of comparison. For this purpose we shall assume for our base or level of reference a surface parallel to the surface of the ocean, and passing through the deepest parts of the ocean-bottom. The following geometrical relation is then apparent.

The volume of the sea above the base level = the area of the whole surface of the globe × the depth of the base level below the sea-level – the volume of rock displacing water between these levels.

Let S = the area of the sea,
D = its mean depth,
L = the area of the land,
W = the volume of the land above the sea-level,
d = the depth of the base level below the surface of the sea,
H = the mean height of the elevations above the base level.

Since $4\pi r^2 H$ would be the volume of the layer spread over the base level, which all the elevations would form, if levelled down; the volume of rock displacing water between the base level and the sea-level will be $4\pi r^2 H - W$; and then our geometrical relation gives,

$$SD = (S + L)\,d - (4\pi r^2 H - W),$$

whence
$$H = \frac{(S + L)\,d + W - SD}{4\pi r^2}.$$

But $4\pi r^2$, being the area of the globe, is the same thing as

$$S + L.$$

$$\therefore H = d - \frac{\text{volume of sea} - \text{volume of land}}{\text{area of globe}}.$$

The area of the ocean is estimated by Dr Haughton to cover 145 millions of square miles, and that of the land 52 millions, and the mean height of the land is 1000 feet[1]. The Challenger soundings give the mean depth of the Atlantic at about 2500 fathoms. In places its depth is 3000 fathoms or a little more, and in one place 4000 fathoms. The greatest depth anywhere is probably in the Pacific, where in one place it is 5000 fathoms. If we adopt Prof. Haughton's estimate of the areas of sea and land, and of the mean height of the land, and estimate the mean depth of the sea at 2500 fathoms; and if we take 4000 fathoms as the depth of the more profound parts, (though we might be entitled to take it greater) we get for the mean height of the elevation above the base level of the deepest parts,

$$H = 13224 \text{ feet}.$$

If we make the assumption that the inequalities of the earth's surface are wholly due to horizontal compression, arising

[1] Haughton, "Proc. Roy. Soc.," vol. XXVI., p. 53, 1877.

from some cause which has affected the whole crust, in that case it would not be probable that the depths of the ocean which we have called the "base level" would anywhere exactly coincide with our "datum level[1]." But if the deepest parts of the ocean do not coincide with the datum level they must be one of two things. They must be either depressions below it, belonging to the series $\Sigma (B)$ of Chap. VII., or else they must be the places where the ocean bottom is least raised above the datum level. That they should be depressions beneath that level is scarcely possible under the assumption that the earth has cooled as a solid; whence we have concluded that $\Sigma (B) = 0$[2]. But if we take the other alternative, and suppose the ocean bottom to be raised above the datum level, even where the depths are very great, then our estimate for H will be too small. And this appears by far the more probable case.

When we compare 13,224 feet with the mean height of the elevations, which could be formed by compression upon a solid earth, owing to contraction through cooling merely, for that, as shown in Chapter VIII., would give no more than $6\frac{1}{3}$ feet on a most liberal estimate, it is obvious that this cause can go no appreciable way towards accounting for the existing mean elevation above the lowest places in the ocean bottom.

In the former edition of this work it was argued that, at the epoch when the crust first solidified, there were neither elevations nor depressions upon its surface; or, in other words, that the surface was one of equilibrium, like that of the ocean. And if the crust passed from a liquid, through a viscous, to a solid condition, this seems to be the natural supposition. And this supposition is strengthened by the fact that all the rocks we see at the surface have either been water-deposited, or else have been protruded through water-deposited rocks. So that no evidence of any primitive inequalities can be adduced, which is opposed to this natural supposition.

But such reasoning was objected to in an able review, which appeared in the "Nation" newspaper, published[3] at New York, in which the reviewer said, that "In spite of the assertion that no evidence can be adduced which is opposed to the supposition

[1] See pp. 85, 111. [2] p. 87. [3] June 15, 1882.

that the crust was without inequalities when it first solidified, such a supposition will appear to many in the highest degree unlikely. The earth is unquestionably a mass of heterogeneous materials of greatly differing densities. While the globe was still in a highly fluid state, an approximate separation into concentric layers, arranged according to their specific gravity, no doubt occurred; but as the temperature of solidification was approached, other physical properties of the constituents must have come into play. Some substances become viscid at much higher temperatures than others, and there is a great difference in the melting-points of various rocks. Layers of equal density must therefore have been far from uniform in their mobility. As long ago as 1879, Mr G. H. Darwin showed ('Philosophical Transactions,' p. 589) that while the earth was still somewhat viscous and not perfectly homogeneous, the attraction of the moon must have induced wrinkles on its surface, of a form suggesting the present continental areas; and he strongly inclines to the opinion that the distribution of land and water, both on the earth and Mars, is attributable to this cause. Though the tops of the highest mountains are some ten miles above the lowest sea-bottoms, the elevations and depressions of the earth's surface bear, after all, a very small relation to the size of the globe—a relation which has been aptly illustrated by comparison with the slightly uneven surface of an orange. That variations of form of this order should have occurred on the surface of the globe at solidification, does not seem *à priori* improbable, even without regard to Mr Darwin's argument, and observations have been made which tend to confirm such a view historically. The regions underlying continents are certainly less dense than those beneath the ocean, and there must have been a similar difference as long as oceans and continents have coexisted. There are also strong geological grounds for believing that continental areas have always been confined to the same regions of the globe as at present, and though this does not necessarily imply that the inequalities were pristine, it at least lends such a supposition considerable probability. In short, the hypothesis of an originally smooth globe requires stringent proof, while Mr Fisher assumes it as self-evident."

The two objections here made to the supposition of a primitive smooth surface on a globe, which had become solid throughout, seem rather difficult to separate. One of them is Professor Darwin's theory of primitive wrinkles, which are supposed to be still represented by the existing continents. The other appears to be, that, "at solidification," portions of different densities would not all freeze simultaneously, and that those of less density might probably be gathered together in some places, and form continents, which are less dense than the parts underlying the oceans. To use a homely illustration, this idea seems to be analogous to the manner in which grease gathers together into patches, and solidifies, on the surface of cold gravy. In this case, surface tension is the cause of the separation. But even here, if the quantity of the lighter substance is sufficient, it cools into a cake of uniform thickness, which completely covers the whole surface. Except under the action of surface tension, which would not be applicable to explain the earth's great features, there is a difficulty in conceiving a segregation side by side of masses of different magmas out of a fused general mass, however originally arranged. A final arrangement in concentric layers seems much more probable.

Turn we now to Professor Darwin's theory of primitive continental wrinkles. He writes "There is an unequal distribution of the tidal frictional couple in various latitudes. We may see in a general way that the tidal protuberance is principally equatorial, and that accordingly the moon tends to retard the diurnal rotation of the equatorial portions of the sphere, more rapidly than that of the polar regions. Hence the polar regions tend to outstrip the equator, and there is a slow motion from west to east relatively to the equator." He then suggests that, owing to the smallness of the effect, this cause has had little or nothing to do with the crumpling of strata, at least within recent geological times. "If however, the views maintained in the paper on 'Precession' as to the remote history of the earth are correct, it would not follow, from what has been stated above, that this cause has never played an important part; for the rate of the screwing of the earth's mass

varies inversely as the sixth power of the moon's distance multiplied by the angular velocity of the earth relatively to the moon. And, according to that theory, in very early times the moon was very near the earth, whilst the relative angular velocity was comparatively great. Hence the screwing action may have been once sensible."

"Now this sort of motion, acting on a mass which is not perfectly homogeneous, would raise wrinkles on the surface, which would run in directions perpendicular to the axis of greatest pressure."

"In the case of the earth the wrinkles would run north and south at the equator, and would bear away to the eastward in northerly and southerly latitudes; so that at the north pole the trend would be north-east, and at the south pole north-west. Also the intensity of the wrinkling force varies as the square of the cosine of the latitude, and is thus greatest at the equator and zero at the poles. Any wrinkle, when once formed, would have a tendency to turn slightly, so as to become more nearly east and west than it was when first made."

"The general configuration of the continents (the large wrinkles) on the earth's surface appears to me remarkable when viewed in connection with these results[1]." The general trend of the coast lines is then considered in this connexion.

The Abstract of the paper in the "Proceedings" states the conclusion somewhat more guardedly, "An inspection of a map of the earth shows that the continents (or large wrinkles) conform more or less to this law. But Professor Schiapparelli's map of Mars is more striking than that of the earth when viewed in the light of this theory; but there are some objections to its application to the case of Mars. If, however, there is any truth in this, then it must be postulated, that after the wrinkles were formed the crust attained sufficient local rigidity to resist the obliteration of the wrinkles, whilst the mean figure of the earth adjusted itself to the ellipticity appropriate to the slackening diurnal rotation; also it must be

[1] "Tides of a viscous spheroid." "Phil. Trans.," part II., 1879, p. 588.

supposed that the general direction of the existing continents has lasted through geological history[1]."

With regard to the above theory taken as a whole, the present writer cannot claim to be among those "few readers, who have gone through the somewhat [*qu.* very] complex arguments and analysis by which the conclusions are supported." But, if he is not mistaken, the earth is taken to be an incompressible somewhat viscous solid sphere[2]. (This it will be observed is opposed to our view of the applicability of Henry's law.) The moon then raises two tidal protuberances of rocky matter, which follow her daily round the earth, the moon being at that epoch very near the earth, and the day consisting of a very few of our present hours. These protuberances, owing to the viscosity of the earth, will somewhat lag behind the moon. The protuberances are necessarily highest at the equator. Having made them, the moon, taking advantage of their lag to get a leverage, or purchase, on them, uses them as if they were handles to hold back the earth as it spins from west to east. She gets more leverage on the parts of these handles near the equator, where, besides, they are larger, and the attraction acting on them therefore greater. The absolute speed of the earth's surface is also greatest at the equator. The combined result is, that there is a twisting effect produced, causing the superficial crust to shear from east to west over the body of the earth. And this effect is greatest at the equator. There will thus be a slow shear, or flow, of the surface over the interior from east to west, and this shear will be most considerable at the equator, and none at all at the poles. Now if the substance was homogeneous, this shear or flow would not affect the smoothness of the surface; but if the matter at the surface gets checked, through being more stiff in some places than in others, there will be an accumulation there (as when a running crowd meets with some obstacle), and thus wrinkles would be formed. These supposed wrinkles must not be confounded with the tidal protuberances themselves, from which they differ altogether.

[1] "Proceedings of Roy. Soc.," No. 191, 1878. I. *Secular distortion of the spheroid.*

[2] "Phil. Trans.," *loc. cit.* p. 548.

It may be replied to this theory, that the formation of the existing continents cannot be looked at apart from their geological history, and that they are evidently dependent on, and as it were gathered around, the great mountain ranges in which they culminate. Although these ranges primarily originated long ago in very early geological times, their present loftiness is due to quite late movements; and, if these had not subsequently occurred, they would before now have probably been razed to the sea-level and have disappeared, so that, whatever cause it was which wrinkled the continents seems to have continued active to times comparatively, if not quite, recent; and the moon is too far off now. The occurrence of great changes of level at no very distant geological period, are manifest from such instances as that related by the elder Darwin—"The land from Rio Plata to Tierra del Fuego, a distance of 1200 miles, has been raised in mass (and in Patagonia to a height of between 300 and 400 feet) within the period of the now existing sea shells. The old and weathered shells left on the surface of the upraised plain still partially retain their colours. The extinct tertiary shells from Port S. Julian and Santa Cruz cannot have lived, according to Prof. E. Forbes, in a greater depth of water than from 40 to 250 feet; but they are now covered with sea deposited strata from 800 to 1000 feet in thickness. Hence the bed of the sea in which these shells once lived must have sunk downwards several hundred feet to allow of the accumulation of the superincumbent strata[1]," and of course has been lifted up again to allow of their being now visible. Other instances of the same kind in abundance will occur to every geologist.

Neither are such comparatively recent changes of level confined to continental areas. Even in the open Pacific Dr Guppy has discovered in the Solomon Islands evidence of elevation to the extent of 12,000 feet distinct from the mere piling up of volcanic ejecta[2]. Similar vertical movements have occurred in the Hawaiian archipelago.

It may also be questioned whether in arrangement the con-

[1] "Naturalist's Voyage," 2nd ed. 1845, p. 172.

[2] "Geology of the Solomon Islands," 1887, p. 126.

tinental areas conform so closely with the theoretical wrinkles as Prof. Darwin seems to think. The vast continental area of the Eastern hemisphere can hardly be said to conform to a system of N.E. and S.W. wrinkling. Its master wrinkles which are the key to its conformation are the Pyrenees, the Alps, the Caucasus, and the Himalayas, and these follow a course nearly parallel to the equator, and moreover in accordance with what has been already remarked have been subject to movements comparatively recent. It may be added, that such a cause, as this theory assigns, might be expected to have given rise to continental wrinkles producing features not very dissimilar to one another in the northern and southern hemispheres. But in fact a striking dissimilarity of geographical features does exist between the two. On the whole therefore it seems open to question, whether the theory can be considered to harmonise with the phenomena. But further, when we remember that it presupposes a solid earth, and that on that supposition such continental wrinkles as Prof. Darwin suggests would probably have been shaped out, this want of congruity with nature of itself goes some way to throw a doubt upon the hypothesis of solidity.

Mr Davison however accepts the above theory, and endeavours to account for the depression of the ocean beds in accordance with it. He thinks that the "stretching" which would take place below that level of no strain discovered by Mr Reade and himself, has been chiefly operative beneath the oceans. "Owing to the pressure of the continental wrinkles, the amount of stretching under them must have been very much less than under the great oceanic areas." This is by no means obvious: for this stretching is the result of a molecular action accompanying cooling, which would go on at the place where the cooling itself took place, and consequently beneath the wrinkles as well as elsewhere. At the same time, their additional superincumbent weight would rather increase the horizontal extension of the subjacent matter than diminish it, for it would add to the "compressive extension" of Mr Reade. Mr Davison proceeds "Thenceforward, therefore, *crust-stretching by lateral tension must have taken place chiefly beneath the ocean-basins, deepening them*

and intensifying their character[1]." But even supposing this excess of stretching to have occurred at the beginning (and, as just stated, we cannot see why it should have done so) Mr Davison does not explain why the effect should continue to be in excess under the same areas, in which case it would lead, as he says, "to the continual subsidence of the ocean bed:" whereas we know that, granting the oceanic areas to have been permanent in position, there are regions in them where the bottom has been at intervals elevated to no inconsiderable extent[2] although not sufficiently to render the water shallow.

But, in spite of all these objections, we have thought it well to calculate the mean depth of the depressions which might be caused in this manner[3], and, as regards amount, the result seems at first sight not unfavourable to the theory. But that does not prove anything, for it must be remembered that the mean depth (η) which we have found to be from 4 to $1\frac{1}{4}$ miles, according as we take the temperature of solidification at 7000° F. or 4000° F., is a superior limit of the depth; and supposes the culminating points of the surface to have altogether escaped depression, and subsequently never to have been lowered by denudation or otherwise, which, as it may be understood by an inspection of the diagram, has never been the case with the points A and B[4]. Under the most favourable circumstances theoretically possible, the depth below the culminating points in the two cases supposed could not exceed 4 or $1\frac{1}{4}$ miles, and in either case might be anything less, and even nothing.

[1] "Phil. Trans." vol. 178, p. 241. Mr Davison couples with Prof. Darwin's researches, a very able article by Prof. Price, published in "Nature," vol. III. p. 315. This article foreshadows several of the problems subsequently completely worked out by Prof. Darwin. Mr T. Mellard Reade has replied to Mr Davison's theory in the "Philosophical Magazine" for August, 1887, p. 213.

[2] See p. 118. [3] See p. 109. [4] See p. 89.

CHAPTER X.

HYPOTHESIS OF SOLIDITY FAILS.

Summary of results of Chap. VIII—Grounds on which Physicists have restricted geological time within certain limits—Comparison of results of Chap. IX. respecting the actual inequalities of surface with those of Chapter VIII. calculated on the hypothesis of compression of a solid globe—Mr Davison's theory of the production of ocean basins—Captain Dutton's argument—Land and Oceans—Opinions of Pratt, Mallet, Hopkins, LeConte—Radial contraction cannot explain ocean basins—Still less can it explain oscillations of surface—N. Wales, Appalachians, New Zealand, India, Colorado Plateau—Theory of Herschel and Babbage—Revived by Reade—Prof. Tait's view of condition of Interior—Replied to—Geological conditions which must be fulfilled—Require a yielding substratum—Crust in approximate hydrostatic equilibrium—Prof. Claypole's account of movements in Pennsylvania—Conditions of equilibrium—Substratum must be liquid—*The whole argument based on* geological investigation.

THE results at which we arrived in the VIIIth. chapter were, that if the elevations on the earth's surface had been caused by compression of the crust due to the contraction of a hot solid globe cooling by conduction to, and radiation from, its outer surface, then the average height of such elevations above a datum level, defined by the matter being perfectly compressible, can be expressed by a formula which we have obtained[1]. If we introduce into this formula the supposition, that the average rate of increase of temperature near the surface is, as experience shows, 1° F. for 51 feet of descent, and if we assume that the temperature at which the globe solidified was 7000° F., then we arrive at the result that the average height of the elevations produced by compression from the era of solidification to the present time would be no

[1] p. 102.

more than $6\frac{1}{3}$ feet. But if instead of 7000° F. we suppose that the globe solidified at 4000° F., which it may be observed is above the temperature of melting platinum, then the average height of the elevations would be only 8 inches[1]. Under the same circumstances, assuming the temperature of 7000° F. as that of solidification, the cooling at the present time would have sensibly penetrated to a depth of about 160 miles, and with a temperature of 4000° F. to about 100 miles. So far none of these results require a knowledge of the conductivity of the materials of which the globe is composed, but merely assume it to have a mean constant value[2].

If the conductivity is known, then the time elapsed since solidification took place is also known, and if we accept the value for this constant found for certain rocks *in situ* at Edinburgh by Sir Wm. Thomson, viz. 400[3], it appears that it would have been about 98,000,000 years ago, if the temperature of solidification was 7000° F.; and about 33,000,000 years if it was 4000° F[4]. These are the grounds, and these are the data,

[1] In the former edition of this work the average heights of the elevations which would have been formed on these two hypotheses were stated to be 900 and 200 feet respectively. The great discrepancy between the former and the present estimates arises from the fact that they were then calculated without reference to the level of no strain, which has been since discovered by Mr Reade and by Mr Davison. The further reduction below the amounts given in the "Phil. Mag." is due to a correction in the mode of calculating ϕ on p. 101. The argument against the contraction theory appears to be considerably strengthened by these alterations.

[2] Principal Forbes found that the conductivity of iron diminishes as the temperature increases. Tait's "Recent Advances in Ph. Science," 2nd ed., p. 264. If such be the case with the earth's crust, then the temperature would increase more rapidly. This would make the average height of the elevations less.

[3] "Trans. Royal Soc. Edin.," vol. XXII., 1860; "Nat. Phil.," p. 718.

[4] To find the time since the earth was all melted, on the supposition that it has cooled as a solid, we have the relation,

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa}}\frac{1}{t^{\frac{1}{2}}}\epsilon^{-\frac{x^2}{4\kappa t}},$$

$$= \frac{V}{35\cdot 4}\frac{1}{t^{\frac{1}{2}}}\epsilon^{-\frac{x^2}{1600t}}.$$

Taking 1/51 of 1° as the rate of increase of temperature per foot at the surface where $x=0$, we get from the above, if $V=7000°$ F., $t=98$ millions of years. If $V=4000°$ F., $t=33$ millions of years. See p. 66.

upon the strength of which physicists have restricted geologists to a term of years for the evolution of the present state of the earth[1]. If the earth be not solid the age of the world cannot be determined in this manner.

We have likewise, in the preceding chapter, estimated the average height of the elevations which actually exist upon the earth's surface above a level of reference coinciding with the deepest parts of the ocean, and found it to be about 13,000 feet or nearly $2\frac{1}{2}$ miles. It is obvious therefore that the larger inequalities of the earth's surface can have little to do with elevation caused by compression of a solid globe through cooling; because even if we confine our attention to the land surface we find that the mean elevation of the land above the sea level is about 1000 feet, and the greater part of the area of the continents exclusive of mountainous regions is not very much elevated; so that this mean elevation is chiefly due to the disturbed strata of mountain chains, and accordingly we see that even that part of the inequalities, which belongs to mountain ranges only, is far too large to be attributable to compression through cooling. Even if we could localize the compression to continental areas, which the assumption of solidity forbids, still we should find the actual so much in excess of the average calculated height, that this device would not bring us appreciably nearer the required amount.

Mr Davison, in his theory already referred to[2], endeavours

[1] "If we suppose the temperature of melting rock to be about 10,000° F. (an extremely high estimate) the consolidation may have taken place 200,000,000 years ago." Sir Wm. Thomson, "Trans. Roy. Soc. Edin.," vol. XXIII., p. 157, 1862, and Thomson and Tait, "Nat. Phil.," p. 716, ed. 1867. "Geology, in framing its conclusions, is compelled to take into account the teachings of other sciences. If we felt disposed to give an indefinite amount of time to the evolution of cosmos out of chaos, as has sometimes been thought possible, the student of heat and mechanics comes in, and asserts upon the authority of his branch of science, that a limit must be put to the time available for bringing about the present condition of things; he will grant 100,000,000 to 300,000,000 of years as the extreme allowance of time; if geologists cannot be content with this allowance, a distinguished professor has said, 'so much the worse for the geologists, for more they cannot have' (Prof. Tait on 'Some recent advances in Physical Science')." Bp. of Carlisle on "Origin of the world, &c.," S.P.C.K. 1880.

[2] Chap. IX., p. 119.

to explain the greater inequalities by a localized "stretching" beneath the oceanic areas, caused by the contraction of a solid globe. We have given reasons why this theory does not appear to be satisfactory.

Captain C. E. Dutton of the United States Geological Survey, long ago noticed the inability of the contraction of a solid globe through cooling to account for the geological facts. His argument is of the same kind as that here adduced[1].

The great problem of the inequalities of the surface naturally divides itself into two provinces; (1) that relating to the land, whose structure we can examine; and (2) that relating to the ocean basins, which cannot be observed. The first enquiry is what the essential character of ocean-beds may be.

Were the earth a perfectly smooth spheroid without any inequalities on its surface, even in that case an excess of density in particular regions would determine a flow of water towards them, and it is conceivable that dry land and oceans might exist, even although the radial distances of the land-surfaces and of the sea-bottoms from the centre of figure might be perfectly equal. That the distribution of the oceans is to some extent actually due to such an excess of density appears certain; for otherwise a whole hemisphere could not be almost entirely covered with water. On this point Archdeacon Pratt remarked, "There is no doubt that the solid parts of the earth's crust beneath the Pacific Ocean, must be denser than in the corresponding parts on the opposite side, otherwise the ocean would flow away to the other parts of the earth." And after explaining the reason for this statement he adds, "There must therefore be some excess of matter in the solid parts of the earth between the Pacific Ocean and the earth's centre, which retains the water in its place. This effect may be produced in an

[1] "American Journal of Science and Art," Jan. 1874, vol. VIII., 3rd Series, p. 113. This article was posterior to the author's first publication on the subject, but entirely independent of it. A subsequent brochure by Capt. Dutton upon the same topic appeared in the "Penn. Monthly," Philadelphia, May, 1876, which was reviewed in the "Geol. Mag.," Decade II., vol. IV., p. 322. See also a review of the first edition of this work by the same writer, "American Journal of Science," April, 1882.

infinite variety of ways; and therefore, without data, it is useless to speculate regarding the arrangement of matter which actually exists in the solid parts below[1]." And Herschel considered that the prevalence of land and water in two opposite hemispheres "proves the force by which the continents are sustained to be one of *tumefaction*, inasmuch as it indicates a situation of the centre of gravity of the total mass of the earth somewhat eccentric relatively to that of the general figure of the external surface—the eccentricity lying in the direction of our antipodes: and is therefore a proof of the comparative *lightness* of the materials of the terrestrial hemisphere[2]."

A like conclusion as to the greater comparative density of the bed of the Ocean, was arrived at by Archdeacon Pratt from the fact that at seven Coast Stations out of thirteen, six being in the Anglo-gallic, and one in the Russian arc, it had been found that a deflection of the plumb-line exists towards the sea. "In fact," he remarks, "the density of the crust beneath the mountains must be less than that below the plains, and still less than that below the ocean-bed. If solidification from a fluid state commenced at the surface, the amount of radial contraction in the solid parts beneath the surface of the mountain-region has been less than in the parts beneath the sea-bed. In fact it is this unequal contraction which appears to have caused the hollows in the external surface, which have become the basins into which the waters have flowed to form the Ocean." Of the fact of greater density there can be no doubt, but it is not necessary to accept this account of the cause of it. Latterly, Pratt attributed the formation of mountainous regions to horizontal compression[3].

Mr Mallet, in his paper on Volcanic Energy, takes a similar view. He thinks that the land- and sea-boundaries were shaped

[1] "Figure of the Earth," 4th ed., p. 236, 1871.

[2] "Physical Geography," § 13, 1862.

[3] In the third edition of the "Figure of the Earth" no mention was made of horizontal compression, but mountains were attributed to vertical expansion. In the fourth, p. 203, note, the author's estimate of the horizontal force of compression is referred to.

out by radial contraction during the first great stage of the operation of refrigeration, while the crust was thin and flexible, owing to the rapid contraction of its viscous portion, which must then have been much thicker than the solid sheet above it[1]; which is not unlike Mr Davison's idea.

Mr Hopkins appears to have been opposed to these views which suppose a difference of radial contraction; and to have held that lateral compression was the cause of the formation of the greater inequalities as well as of the lesser ones, for we find him in discussing M. Elie de Beaumont's theories to have used these words: "The physical cause to which our author refers the phenomena of elevation—the shrinking of the earth's crust—is that which appears to me most unlikely to produce that paroxysmal action which his theory so essentially requires; and most likely to produce those slow and gradual movements which it scarcely recognises. The actual *depressions of the great oceanic basins*, and generally the more widely extended geological depressions of the present or former periods, may, I think, be referred with great probability to this cause[2]."

The theories respecting the formation of the larger features of the earth's surface have been discussed by Professor LeConte in so lucid and unprejudiced a style that his papers are well worthy of study. He attributes the formation of mountain chains to "secular contraction of the interior of the earth"; "all that constitutes scenery to subsequent erosion." "But, as we find no unmistakable structural evidence of crushing except in mountain chains," he "prefers to attribute the formation of continents and sea-bottoms to unequal *radial* contraction[3]."

The last sentence appears to invite the remark that we cannot expect in general to have evidence of "crushing" except in those areas which are open to investigation, viz. on dry land. But *there* it is not confined to mountain-chains. Contorted strata are to be also found in what would be termed level

[1] "Trans. Roy. Soc.," 1873, § 52 and § 60.

[2] Presidential Address to the Geol. Soc. 1853, "Quart. Journ. Geol. Soc.," vol. IX., p. lxxxix.

[3] "American Journal of Science," 3rd Series, vol. IV., p. 462, 1872.

countries, often covered with horizontal deposits of later date[1]: and this fact in itself proves that these contorted strata have been once covered by the sea, offering a presumption that there are contorted strata now at the bottoms of the oceans; at least of such areas as are subject to sedimentation.

In fact it is clear that, on account of the sedimentary character of the rocks that compose continents proving that they cannot be primæval protuberances, the only hypothesis which can compete with that of lateral compression, on the hypothesis of solidity, is that of radial contraction; which may have left the land deserted by the water, which once covered it. But we must remember that, if the ocean basins are due to this cause, it is only the difference of radial contraction between them and the land that can be appealed to, to account for their relative depression. And here we again are brought in face of Mr Davison's theory, which has been already examined and we believe disposed of[2].

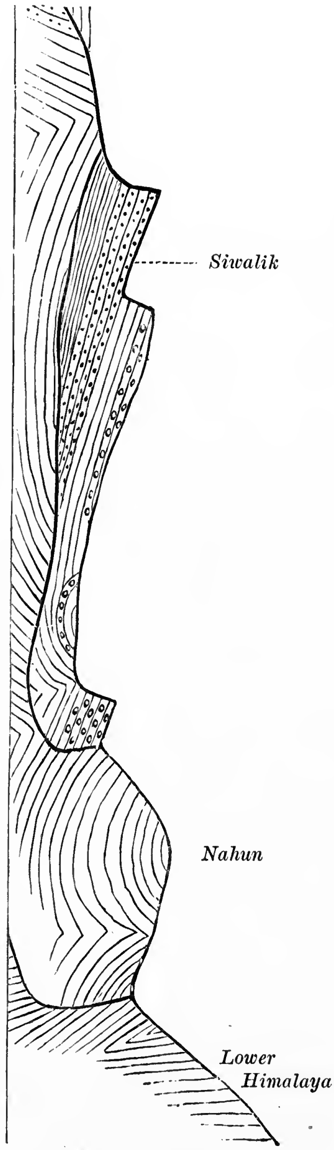
After what has been said it seems hardly necessary to adduce any further arguments against the production of ocean basins by radial contraction owing to cooling merely. But it will be well to recollect, that there are additional difficulties in explaining by this means the *oscillations* of the surface up and down, which are known to have occurred in the same areas again and again: for that land and sea may interchange places by radial contraction, the land must sink far below the bottom of the sea (because the radial contraction cannot cause the latter to rise up) so that the sea might flow into the depression thus caused, leaving its former bed forsaken and dry.

But, setting aside contraction through cooling by conduction, there may be other causes producing locally both radial contraction and also expansion, on which we have already slightly touched in the sixth Chapter[3]. It is also conceivable that the ocean basins may have been directly excavated, and the material which once levelled them up have been removed from this terraqueous globe[4].

The more one considers the instability of the earth's crust,

[1] For example the highly contorted carboniferous strata of parts of Belgium.

[2] p. 120. [3] p. 77. [4] *Vid. infra*, ch. xxv.



After Medlicott, "Quart. Journ. Geol. Soc." vol. XXIV. p. 49, 1868.

and the magnitude, according to our ordinary standards, of the movements which have occurred, the more one is lost in amazement. And it is impossible to avoid using the kind of exaggerated language which is the opprobrium of popular geology. Rocks of a single geological period, thicker than the heights of the highest mountains, have been deposited over certain areas, sunk, been re-elevated, crumpled, and denuded, and still stand as lofty mountains. In North Wales, Cambrian rocks were measured by Mr Aveline of the thickness of 23,000 feet[1]. The deposits of the Appalachian chain, we are told, attain a thickness of eight miles[2]. In such cases as these, not only must the rocks now exposed at the localities, where the lower beds of the series are at the surface, have been at one time as far below the sea; but neighbouring areas now occupied by sea must have presented land surfaces, in the latter instance believed to have been to the east, in order to furnish the detritus out of which these great piles of strata were formed. Again the islands of New Zealand occupy a central position in the aqueous hemisphere; and yet they contain a series of deposits chronologically analogous to those of the northern hemisphere[3]; whence it follows that they must have been often submerged during periods, when there existed, in what is now an extended ocean, land surfaces not far distant, from which the materials of their rocks must have been derived.

The Himalayan area presents some peculiarly interesting features in this connection. The sub-Himalayan range consists of tertiary strata, which are now highly disturbed. All, with the exception of the lower portion, which is nummulitic and consequently marine, are composed of sub-aerial deposits, formed by detritus brought down by torrents from the Himalayas. These deposits are together between 12,000 and 15,000 feet thick. The sandstones, which form the chief portion of these beds, and the red clays which are intercalated with them, are exactly like

[1] Jukes' "Student's Manual of Geology," 3rd ed., p. 526, 1872.

[2] Hall's "Natural History of New York," part VI. Palæontology, vol. III., p. 67.

[3] Capt. F. W. Hutton, "Geol. Mag.," Decade II., vol. I., p. 27, note. See also Hochstetter's "Geology of New Zealand," Auckland, 1864. For a list of the strata in New Zealand see Prestwich's "Geology," vol. II., p. 17, 1888.

the alluvial deposits of the plains. "Thus it was suggestive, and not altogether misleading, to say that the Siwaliks were formed of an upraised portion of the plains of India[1]." The surface movements indicated by this history suggest a level area at the foot of the Himalayan range, sinking continuously during the former part of the tertiary period. Then a great movement of lateral compression and elevation took place. Again it sunk, and unconformable beds were deposited. These were again elevated and compressed. Such at least appears to be the interpretation of the description given by the surveyors. But the point which is material to our present subject is the sinking of the land surface to the depth of nearly three miles, while river deposits to that thickness were being laid down; the whole being denuded off mountains whose spoils have in more recent times provided materials for the great plains of India; and still those mountains stand the highest in the world. That a sinking of the area of the plains of a similar character is yet in progress, is shown by the boring at Fort William, near Calcutta, in which to the depth of 400 feet fresh-water deposits occurred. The conclusion seems irresistible that corresponding to the long, though occasionally interrupted, depression of these plains, a correlative elevation of the great range which has supplied the deposits has been going on.

This sinking of river plains and estuaries is an apparently universal occurrence: the great thickness of fluviatile deposits in them cannot otherwise be accounted for. Thus the fluviatile accumulations have been proved in the case of the Po to the depth of 400 feet, of the Ganges to 481, and of the Mississippi to 630 feet[2].

The recent explorations of the region of the Rocky Mountains by the Government Survey of the United States, has greatly added to our knowledge about the movements of the earth's crust both in kind and in degree. The rocks affected are of many periods, down to the newer tertiary, and even the quater-

[1] "Manual of the Geology of India," Medlicott and Blandford, p. 525.

[2] Dr Ricketts, "Valleys, Deltas, Bays, and Estuaries," p. 22. Liverpool, 1872.

nary[1]. They are cut up by enormous faults into blocks, the throw of the faults being sometimes as much as 40,000 feet. Yet the movements have been so gradual that the rivers have had time to deepen their channels as the plateaux rose, so that they now flow in the profound "Cañons of the Colorado" sometimes more than a mile deep. Great however as is the thickness of the strata of this wonderful region, the sea in which the marine portion of them has been deposited has been shallow all along, showing that the bottom sank as the deposition proceded. Captain Dutton tells us that, "during the entire [mesozoic] age, the surface of deposition was always very near the sea level. The proof of this is abundant and clear. Throughout the entire Plateau Province, the strata are all shallow-water deposits." ..."The mesozoic beds [Permian, Triassic, Jurassic, and Cretaceous] were deposited with almost rigorous horizontality, and very nearly at sea level, throughout the entire mesozoic[2]."

In his dissertation upon the Zuni Plateau the same Author tells us that, "on the whole the region appears to have subsided about as fast as the sediments accumulated, thus preserving the surface nearly at a constant level. We are tempted here to surmise that these oscillations between land and water may have been due to variations in the absolute position of sea level, while the progressive subsidence was due to the weight of the sediments themselves. Whatever may be the explanation, it is a most extraordinary fact that 3000 to 4000 feet of strata were accumulated upon an area of over 90,000 square miles, and yet the surface of deposition was maintained throughout at approximately the same level, or with very moderate variations from it." The above description refers to Permian and Triassic strata; but very similar conditions obtained during the Cretaceous era likewise[3].

If difference of radial contraction would be inadequate to

[1] "High Plateaus of Utah." Capt. Dutton, Prefatory note by Major Powell, p. viii. See a review of this work by Prof. A. Geikie in "Nature," vol. XXII., p. 324.

[2] Dutton on "The Grand Cañon District," p. 70, Washington, 1882.

[3] "Sixth Annual Report of the U. S. Geological Survey," p. 185, Washington, 1885.

account for the existing inequalities of the surface, even if they were stationary, still more is it incapable of accounting for such oscillations as are known to geologists to have affected the surface at many periods.

The theory of Herschel[1] and Babbage[2], since revived with additions in a somewhat different form by Mr T. Mellard Reade[3], that the heat, conducted upwards into the thick deposits laid down at the bottom of the ocean, expands and elevates their surface, is possibly a *vera causa*, but quite incapable by itself of explaining great changes of level. Moreover the heat conducted into the new deposits must be abstracted from the couches beneath, so that there can be no absolute increase in the amount of heat beneath the area in question except such as is supplied to it laterally, so that the process must be excessively slow.

All these phenomena are perfectly well known to geologists, but appear to have been unaccountably ignored by the distinguished physicists, who have discussed the condition of the interior of the earth. They cannot be explained upon the supposition that the earth is solid[4].

It is nevertheless interesting and important to learn the opinion of a pure physicist and mathematician upon these questions. Prof. Tait writes, "Just as the effect of pressure on the melting point of ice enables us to account for the plasticity of glacier ice, so the effects now described" (the sinking of the lava crust on Kilauea) "enable us to explain with great probability, why it is that the materials forming the interior of the earth are practically rigid, and therefore solid, though at temperatures far above their melting-points."

1 "Proceedings of the Geol. Soc.," II., pp. 75, 550. Also Lyell's "Elements," vol. II., p. 231, 1872.

2 "On the Temple of Serapis," Journal Geol. Soc. vol. III., p. 186; a paper containing the germ of much true reasoning about the physics of the earth's crust.

3 "On the Origin of Mountain Ranges," 1886. See a review of this work by the Author, "Geol. Mag." N. S., Dec. III., vol. IV., p. 229.

4 The geological arguments against solidity have been marshalled in a masterly way by Prof. Prestwich. "Geology," vol. II., ch. XXXIV., p. 536, 1888: and more fully in his paper "On the agency of water in volcanic eruptions," § 6, *Thickness of the Earth's Crust from a geological standpoint*, "Proc. Roy. Soc.," no. 246, 1886, p. 158.

"For...there can be little doubt that the temperature, at a few hundred miles below the earth's surface, must be very high —certainly far above the melting-point of lava. Yet, as Sir W. Thomson has shown from the amount of the tides, there can be no doubt that, as a whole, the earth is nearly as rigid as a globe of steel of the same size."

"These two apparently inconsistent conditions are at once reconciled, if we suppose the average materials of the earth to be such as, like lava, to expand on melting;—for the immense pressure, to which they are subjected from superincumbent strata, is probably sufficient to raise their melting point above their present temperature. Thus they may remain solid even at a white heat. But if, by the cooling and shrinking of the lower strata within the solid crust, the pressure should be anywhere considerably relieved, the mass would (almost explosively) melt with considerable expansion. This seems to have important bearings on earthquakes and upheavals[1]."

The above is an attempt to provide a liquid substratum, where surface manifestations obviously show that one exists, without admitting such a condition of liquidity to be generally distributed. It is an explanation of what some writers have called "potential" fluidity. Three fundamental hypotheses are involved. (1) That lava sinks because it contracts on solidifying. This is not certain, as has been already pointed out[2]. Moreover, even if it be the case, it does not prove that a cooled crust, chiefly consisting of acid rocks, would not float upon melted basic rocks, whose density is higher and temperature of fusion lower. (2) It assumes that Sir W. Thomson's proof of the rigidity of the earth is irrefragable; that proof assuming that the materials of the interior are incompressible, which they probably are not. (3) It makes no provision for the sinking of areas concomitantly with the deposition of sediment. For that must increase the pressure beneath the loaded areas, and consequently ought upon the first hypothesis to render the substratum more solid than it was before, and less ready to yield and allow them to sink.

[1] Tait's "Heat," p. 123, 1884.

[2] p. 45, *et seq.*

Besides this capacity of yielding to vertical pressure from overweighting, it is necessary to allow for the very considerable lateral shift, which the superficial part of the crust has undergone, towards mountainous regions. Prof. Claypole infers that, during the formation of the Appalachian mountains, 100 miles of surface have been compressed into about 65 miles; which he considers rather below the truth. This would make the compression 2/3[1].

In short what is required to accord with the known phenomena is a liquid, or at least a plastic substratum, for the crust to rest on, which will allow of local elevations and depressions, and of a certain amount of lateral shift towards mountain ranges. It will then be easy to explain the sinking of areas in proportion to their becoming overloaded with sediment, whether beneath the ocean or sub-aerially. In that case the semi-rigid crust would assume a position of rest upon this substratum, which, within certain limits depending upon the rigidity of the crust, would be one of hydrostatic equilibrium. That is to say, the distribution of load might be made *somewhat* different from what it is without disturbing the equilibrium, but it could not be made *very* different. For instance, if the present configuration of the Himalayan region be one of approximate equilibrium, which it clearly is, if much sediment is brought off the mountains and spread over the plains, the mountains become after a while too light and the plains too heavy, and accordingly the mountains rise and the plains sink to restore the contour. This appears to be what has happened.

Prof. Claypole, accounting for the enormous palæozoic deposits of the Eastern States says, "It is not probable that the land [whence the sediment was derived] ever stood at any dizzy height above the sea. The slow secular depression, of which we have so abundant evidence from every part of the globe, and which in this very region has carried Middle Pennsylvania down at least thirty thousand or forty thousand feet below its former level, is quite sufficient to account for the phenomena. Let but the sea margin, which was the loaded area, sink somewhat faster than the dry land, which was the lightened

[1] "American Naturalist," March, 1885. See *Report of Brit. Assoc.* in "Geol. Mag.," Dec. III., vol. I., p. 466, 1884.

area, and we have all the conditions that were needed for the erosion and denudation of the relatively rising surface to any amount. The only limit is some physical change, which could put a stop to the rising of the one and the sinking of the other, and this came at the end of palæozoic times, when the Appalachian trough became full[1]"—namely the trough, in which the thick sediments accumulated, which were shortly after crumpled up into the Appalachian Range.

Here then we have an instance in palæozoic times of what has occurred much more recently in the Himalayan area. Hydrostatic equilibrium was disturbed. The lightened area rose and the loaded sank.

The forces under which any portion of the crust will be in equilibrium will be (1) the tangential stress, (2) its own weight, and (3) the upward pressure of the substratum. If any one of these is changed beyond a certain amount, the equilibrium will be destroyed, and a movement of the crust will ensue.

Here are three forces involved, each of which is due to a different proximate cause: the tangential pressure producing compression of the crust (from whatever arising); the distribution of the weight of the crust, which will depend upon the transfer of sediment, and is therefore caused partly by solar energy; and pressure from beneath, which may be attributed to elastic matter confined by the superincumbent crust.

No true theory of crust movements can be propounded, which does not involve all these simultaneously. But it is hardly unfair to say that in general this consideration has been overlooked, and one or other, or perhaps two together of these causes, have been invoked, and the theory has been incomplete accordingly.

We henceforward assume the existence of a substratum which is continuously and continually fluid.

This may be considered a bold and extreme demand, for it may be asked why a somewhat viscous or slightly plastic substratum, would not meet the requirements. It is certain that stones which are solid and hard, such as we see and handle in rock specimens, show signs of fluxion structure, which there

[1] "American Naturalist," vol. xxi., Nov. 1887, p. 960.

is every reason to believe they have acquired, while in what we should commonly designate a condition of solidity. Viscosity is a property in which time is an element involved, and with sufficient time given, bodies of great hardness may be affected by a viscous shear: for there is no such thing as a perfectly rigid body. Nevertheless our reasoning appears logically to involve the necessity of a mobile substratum of considerable liquidity; for which we argue as follows.

Geological phenomena, such as have been described in the present chapter, appear to require a thin crust resting upon a yielding substratum. Physicists tell us that a yielding substratum is not admissible, because it would be affected by tides. We think however, that we have discovered a way out of this dilemma in our fifth chapter, where we have shown that a liquid substratum, holding gas in solution, need not necessarily exhibit tides superficially.

So far then we obtain a thin crust upon a yielding and hot substratum, which probably holds gas in solution. But in our sixth chapter we have advanced a step further still; for we have found that the crust cannot be thin, unless the substratum which underlies it is constantly employed in preventing it from growing thick by dissolving off, or as it were washing away, its under side; not as fast as, but nearly as fast as, it solidifies. The proof of this appears to be complete. Now there must be considerable freedom of movement, of a kind which we may call spontaneous, in such a substratum. In short it must be affected by convection currents, and therefore it must be liquid.

It will be noticed that the above reasoning is primarily based upon the results of *Geological investigation* as to the nature of the movements to which the visible parts of the crust have been subjected, and upon the conclusion drawn from these, that they indicate a thin crust resting on a yielding substratum, and the result follows that the yielding cannot fall short of flowing.

CHAPTER XI.

LIQUID SUBSTRATUM.

*Theory of a liquid substratum partially held by Scrope—Sorby's discovery of water in granites—Igneo-aqueous fusion—Ebullition would not continue—Manner of combination of fused rock with water—Emission of steam from Volcanos — Vesuvius — Krakatão — Tarawera — Bandai-san — Kilauea—Opinion that surface water gains access to foci—Objections to this—Experiment of Daubrée not in point—Views on cosmogony—Dr Sterry Hunt's view—Objections to it—A suggestion to explain the presence of water—Water dissolved in rock, rather than rock in water—*A priori *probability that such a substratum would be produced—Alternative would lead to Davy's theory of underground temperature.*

The fifth chapter contained a suggestion, that the substratum of the earth's crust consists of molten rock, holding gas in solution in accordance with Henry's law, and that this gas consists chiefly of water-substance, which will of course be above the critical temperature of 700° F. This is little more than an extension of the theory held long ago by Scrope, who, describing Stromboli and Masaya, says, "there unquestionably exists, within and below the volcanic vents, a body of lava of unknown dimensions, permanently liquid at an intense temperature, and continually traversed by successive volumes of some æriform fluid, which escape from its surface—thus presenting all the appearances of a liquid in constant ebullition[1]." "If any doubt should suggest itself, whether this [elastic] fluid is actually generated within the lava, or only rises through it, having its

[1] "Volcanos," 2nd ed., 1862, p. 34.

origin in some other substance or in some other manner, beneath, it must be dispelled by the evidence afforded in the extremely vesicular or cellular structure of very many erupted lavas, not merely near the surface but throughout their mass, showing that the æriform fluid in these cases certainly developed itself interstitially in every part[1]." Many will think that this view of the essential nature of volcanic action has not since been improved upon. We have merely indicated in addition the law, according to which the combination of the gas with the liquid lava may probably be governed, and suggested that the "unknown dimensions" of the reservoir extend beneath the entire crust of the globe. Indeed Mr Sorby's observations on the water enclosed, along with crystals of chlorides, inside the constituent crystals of granites, renders it apparent that the steam emitted in eruptions may be a constituent part of the deep-seated rocks, for it is probable that but a small part of the water contained in any magma would become confined in the interior of the crystals[2].

Here, however, the question arises whether it would be possible for a crust to form over a layer of molten rock in a condition of igneo-aqueous fusion. Would not the escape of the water cause a state of constant ebullition which would prevent the formation of any crust, until it had ceased through the escape of all the water?

Henry's law here comes to our aid, and teaches us, that, at depths where the pressure was sufficient, there would be no tendency to the formation of vesicles and consequent ebullition, so that, after a certain quantity of water had escaped to a corresponding depth, ebullition at the surface would cease, and a crust begin to be formed. Convection and diffusion from the depths below would however constantly tend to restore the abstracted water, so that it is probable that, at the bottom of the solid crust when once congealed, there would always be found magma so fully saturated with water, that the least diminution of pressure would cause the water to separate into vesicles, and so exercise an elastic pressure beneath the crust.

[1] "Volcanos," 2nd ed., 1862, p. 36.
[2] "Quart. Journ. Geol. Soc.," vol. xiv., p. 453.

A paper by the Author, "On the inequalities of the earth's surface viewed in connection with secular cooling," published in 1873, contained the following passage[1]:

"If such was the condition of the interior in the early stages of the cosmogony, a large portion of the oceans now above the crust may once have been beneath it, and thus we gain a novel conception of a sense in which the fountains of the abyss may once have been broken up," and a suggestion was added, that the nucleus might have been considerably diminished in size, owing to the loss of this water; an opinion which, as will appear further on, had to be abandoned[2].

Upon this passage the Referee[3] made the following annotation.

"The Author suggests other causes of shrinking besides loss of heat, namely the escape of water.

"It is probable, or rather certain, that water substance, if it exists at great depths under great pressure and at high temperature, is neither a gas nor a liquid, being above its critical point.

"In this state substances are easily dissolved in it, not however so much on account of a greater tendency to combine with water, as on account of a greater tendency of their own to dissipation. At still higher temperature the water substance becomes itself dissociated into oxygen and hydrogen. But it does not follow that the dissolved substances will be precipitated. The magma may be all the more complete the higher the temperature, because, though the bonds of affinity have fallen away, the prison-walls prevent the elements from escaping. But of all the known regions of the Universe the most unsafe to reason about is that which is under our feet."

Still further—"Water under pressure in the magma may however be chemically combined, and not merely in solution, since pressure has precisely the same kind of effect in keeping substances combined, when one of them is at lower pressures

[1] "Trans. Cambridge Phil. Soc.," vol. XII., pt. 2, p. 431, 1873.

[2] Mr Scrope received this suggestion with much favour but did not live to consider the further discussion of the question.

[3] Possibly the late Prof. W. H. Miller.

gaseous, that it has in keeping gases in solution. Indeed at 2000° C. (3632° F.), water-vapour at ordinary pressures would be in great part separated into hydrogen and oxygen, though with high pressure the decomposition might be almost wholly prevented[1]." And Hannay states that even delicate compounds may, by sufficient heating, be rendered gaseous without decomposition, if under sufficient pressure[2].

From whatever source it may be derived, there can be no question regarding the fact, that enormous quantities of steam are emitted from volcanos when in eruption; and some amount almost continually from many. To be assured of this, it is sufficient to look at the frontispiece of Scrope's "Volcanos," or at a photograph of Vesuvius on the 26th April, 1872[3]. Palmieri relates how on that day "two large craters opened at the summit, discharging with a dreadful noise, audible at a great distance, an immense cloud of smoke and ashes, with bombs and flakes, rising to the height of 1300 metres above the brim of lava." The Author remembers that in the narrative of a correspondent of a newspaper at the time, the sound, as heard at Naples, was compared to four lions roaring in the four corners of the room in which he sat.

[1] Extract from a private letter to the Author by Prof. Liveing, 1887.

[2] "Nature," vol. XXVI., p. 370, 1882. Earlier experiments led Mr Hannay to the following conclusions:—

"The general result obtained from these experiments was, that the solvent power of water was found to be determined by two conditions: (1) Temperature, or molecular *vis viva;* and (2) Closeness of the molecules on pressure, which seems to give penetrative power. From these observations it will be seen that, if a body has any solvent action on another and does not act upon it chemically, such solvent action may be indefinitely increased by indefinitely increasing the temperature and pressure of the solvent. In nature the temperature has been at one time higher than we can obtain artificially, and the pressure obtained by a depth of 200 miles from the surface is greater than can be supported by any of the materials from which we can form vessels. It will thus be seen that, whereas in nature almost unlimited solvent power could be obtained, we are not as yet able to reproduce these conditions artificially. Could pressure alone increase solvent power, then much might be done, but pressure only acts by keeping the molecules close together when they have great *vis viva;* and this latter is only obtained by high temperature."—Hannay on the Artificial Formation of the Diamond. Paper read at the Royal Society. See "Nature," July 15, 1880.

[3] Reproduced in Palmieri's "Vesuvius," translated by Mallet, Asher and Co. London, 1873. Also in Judd's "Volcanoes," p. 24.

Since the first edition of this work was published in 1881, we have passed through a period of remarkable volcanic and seismic disturbance. The tremendous eruption of the volcano in the Island of Krakatão in the Straits of Sunda[1] exemplified the immense explosive power of the steam. It is estimated that matter was shot up to the height of 50 kilometres; that is nearly six times as high as Mt. Everest, the loftiest mountain in the world[2]. The dust carried into the higher atmosphere lingered two years, causing the coppery hue of sky, which should have been blue, around the sun, strikingly seen even in our own latitude, and known as "Bishop's ring." The aqueous vapour also produced those remarkable glows after sunset, whose beauty will be long remembered.

The eruption of Mt. Tarawera in New Zealand, 10 June, 1886, though not comparable with that of Krakatão, was in some respects more remarkable. The mountain was not known to be a volcano, and had nothing of the shape of one; when suddenly it burst open. "As witnessed from Taupo, 30 miles to the south, the first intimation was a continuous rumbling, followed by a sharp report. A large ball of fire was seen to be ejected to an immense height, and burst, sending out a shower of sparks and dense volumes of smoke. This was followed by a cloud rising above our horizon[3], mushroom shape, densely thick on the top, lighter towards the horizon, scintillating and sending out sparks of apparently electric nature in every direction. The lower part occasionally opened into flame, but the whole was in incessant fiery agitation[4]."

Again, on the 15th July, 1888, Little Bandai-san, a mountain in Northern Japan 5800 feet high, which had shown no signs of activity for eleven hundred years, was suddenly blown into the

[1] August 27, 1883.

[2] Verbeek's "Krakatão," reviewed, in "Nature," Oct. 22, 1885. At a distance of 30 miles at sea "the roarings of the volcano were fearful." Letter from Capt. W. J. Watson, "Nature," Dec. 6, 1883, p. 141.

[3] This shows that the mountain itself was below the horizon of the observer. What therefore seemed to him at a distance of 30 miles but sparks, were no doubt the flashes of lightning that always burst from the steam cloud of a volcano.

[4] "Christchurch Press," June 18, 1886.

air, and obliterated. The debris covered an area half the size of London[1].

These extreme exhibitions of disruptive and impulsive energy accompany those eruptions, which take place at long intervals, during which it appears as if the elastic substance accumulates in quantity, and increases in tension. The volcanos of Hawaii are of a different character. They seem never to have been explosive. The enormous bulk of the mountain, on which they are situated, is probably due to this cause; for nearly all the ejected matter is retained where it runs out, and none of it is scattered to the winds. The great crater of Kilauea is totally unlike a crater within a cone. Its area of nine square miles is out of all proportion to its depth. When in a moderately active state, jets of liquid lava, about thirty feet high, play, wandering about on the surface, but keeping mostly near the edges of the molten pool, which is like a thawed place in a sheet of ice. It does not appear that any steam visibly arises from these jets, immediate condensation being prevented by the heat which is radiated from the incandescent surface of the lava; but a magnificent cumulus cloud hangs over the crater[2]. This crater varies so much at different times, that descriptions of it by successive observers hardly read like descriptions of the same place.

Mr Green of Honolulu, who has watched the phenomena during a long residence, attributes the cloud, which hangs over Kilauea, to the uprush of the air drawn in from all around, and condensing the atmospheric vapour as in an ordinary rain cloud. But Captain Dutton is decidedly of opinion that the vapour is given off by the lava itself[3]; and such appears to have been Commander Wilkes' immediate impression. And that some kind of gas or another is given off is evident, on account of the ebullient action, and the jetting. Mere convection by difference of temperature, without vesicles involved, could not produce such active movement. On the whole the evidence points to this volcano offering no exception to the general rule, that the

[1] "The Times," 11th Sept., and "Nature," 13th Sept., 1888.

[2] See accounts of this volcano in Chap. IV., especially that by Commander Wilkes, p. 46.

[3] Dutton's "Volcanoes," *Ordnance Notes*, no. 343, p. 14, 1884, Washington.

vapour of water is given off from incandescent lava, only here constantly in smaller, elsewhere, paroxysmally, in larger quantities. The question is, not regarding the fact that water is present in the volcanic emanations, but how it comes to be there.

This of course opens up the whole subject. The circumstance that many of the best-known volcanos are near the sea, that many actually form islands in the wide ocean, and that great trains of volcanos skirt the shores notably of the Pacific, has led to the opinion that the waters of the sea by some means find access to what have been called the volcanic foci, and are there heated, and find exit through their vents. This explanation has been thought to receive support from the "sea salts," which are deposited by the emanations[1].

There are but two ways in which the waters of the sea could possibly find access to the hot interior; and these are, by open fissures, and by capillary absorption. If a fissure were to open at the bottom of the sea, so that water were to gain access to heated rocks, it seems incredible that the steam, if such were formed, should not be formed at once, and the water be forced back again through the same fissure by which it entered. Or if we suppose, as is possible, that the pressure of the water would prevent the formation of steam, still there would be a highly expansive fluid, which would rush upwards at the same place, rather than traverse a long underground journey to find exit at the nearest volcanic vent.

Indeed an instance of what would ordinarily happen if water was admitted to hot lava by a fissure, was afforded during the eruption of Tarawera. After the mountain had burst open, and sent up its column of steam, dust, and red-hot stones, a second great explosion took place, which was caused by a fissure starting from the mountain, and running beneath the hot lake of Rotomahana. Thereupon the contents of the lake were

[1] "Even chloride of iron, which was so abundant in the lavas, was scarcely perceptible in the smoke, which almost exclusively deposited sea-salt on the surrounding rocks; I say sea-salt advisedly, and not chloride of sodium, to show that I include all that sea-salt contains." Palmieri, *Op. cit.*, p. 121.

hurled into the air, and it became the site of immense mud volcanos[1].

Humboldt remarks that Jorullo, Popocatepetl, and the volcano of Fragua are respectively 80, 132, and 156, geographical miles from the sea, and in central Asia, Peschan, and Hotscheu, are in the Thianschan, 1400 miles from the Polar sea, 1528 from the Indian ocean, 1360 from the Caspian, 172 from Issikoul, and 208 miles from Lake Balkasch[2].

The more plausible opinion is, that the water gains access by capillary absorption, and slowly accumulates in the supposed "volcanic foci," until at last it bursts forth in an eruption. Capillary action can be made to do great things, as for instance to split blocks of granite, by driving in dry plugs of wood and wetting them. But it cannot cause a liquid to flow continuously through a tube, however short; for, if it could, it would give us perpetual motion. And after all it is a finite force, and requires special conditions for its development. The chief of these is that surfaces of three media should meet two and two in contact. Thus when water rises in a capillary tube of glass, we have the three media of glass and water and air. The tension of the surface separating water and air is greater than that of the surface separating water and glass[3]. In the present case, there is no third medium present, vapour of water, which might act as such, being prevented from forming by the pressure, so that it does not appear how the action can be set up. If there were a cavity filled with vapour, it is possible that the density of that vapour, and therefore its pressure, might be increased to a certain extent, by the evaporation of the water from the extremity of the capillary tubes, and that was what occurred in the experiment of M. Daubrée[4]. But under the conditions present

[1] See "Report" by Mr Thomas, Gov. Printing office, Wellington N. Z., 1888, referred to in Chap. XXII., *infra*.

[2] "Cosmos," Sabine's translation, vol. I., p. 232, 1842.

[3] See Maxwell's "Heat," 2nd ed., p. 286.

[4] M. Daubrée (*vide* "Rapport sur les progrès de la Géologie expérimentale," Paris, 1867) deduced a theory of volcanos from the following experiment. He separated two chambers by a circular plate of rock two centimetres in thickness. The upper chamber communicated freely with the atmosphere, the plate of rock forming its lower face. The second chamber was empty, and communicated

the pressure is too great for the formation of vapour and the temperature too high for capillary action to be developed[1].

Still further the existence of capillary communication of water from the surface may be doubted. For if there were supposed a capillary tube extending from the bottom of the ocean, the pressure at the lower end of this tube would be that of the water contained in it *plus* that, if any, arising from capillarity, while the pressure of the crust around its mouth would be that due to the weight of the crust. This latter would be the greater of the two: consequently the liquid upon which the crust rested, having a tension equal to the weight of the crust, would force back the water in the tube, and if it were not too viscous would itself occupy the tube[2]. Thus it appears that the

with a manometer. The entire apparatus having been raised to 160° C., water was poured into the upper chamber; and soon afterwards the manometer was found to indicate two atmospheres of pressure. When a portion of the vapour accumulated in the lower chamber had been allowed to escape, the pressure soon recovered its former value. There was then a true feeding across the partition of rock, the cause of which was the desiccation of the lower face of the partition by vaporization of the water in its interstices, and the subsequent replacement of that water by capillary action. (Capillary action acted the part of the force-pump which feeds a high-pressure boiler.)

M. Daubrée conceives that if the layer of rock were of great thickness, and a very high temperature maintained in the cavity, a correspondingly high steam-pressure would result, which would be sufficient to raise lava in the vent of a volcano, and to produce earthquakes; while the force so obtained might after expenditure be again and again renewed.

This theory requires the occurrence of cavities at great depths ("supposons une cavité séparée des eaux de la surface") communicating with the volcanic vents. But the only argument in favour of cavities existing seems to be that the requisite mechanical force is supposed obtainable by means of them; but it seems *à priori* impossible that there should be such cavities.

[1] "In all liquids on which experiments have been made the superficial tension diminishes as the temperature rises, being greatest at the freezing point of the substance, and vanishing altogether at the critical point where the liquid and gaseous states become continuous." Maxwell's "Heat," 2nd ed., p. 290.

[2] On the question of capillary action, Captain Dutton remarks that all "amorphous silicates" at a red heat are pasty. "Now a crack or a fissure might reach very far down into hard, cold, brittle, rocks, but into soft semi-fused rocks, never. Under a pressure of several miles of superincumbent strata, a crack, or even the minutest vesicle, would be tightly closed up, as if its walls were wax or butter." "Volcanoes," p. 17. It may be doubtful whether in solidifying the magma would pass through an amorphous state. But even if it did not, this argument would still have much force.

waters of the ocean cannot supply the steam emitted from volcanic vents. The conclusion seems inevitable that water substance holding salts in solution, is an original constituent of the magma from which these vaporous emanations are derived. What is this magma?

There is a natural unwillingness among geologists to involve themselves in speculations concerning the cosmogony. This is due partly to the inherent uncertainty of the subject, and partly to the deeply implanted doctrines of the uniformitarian school, which in effect teaches that there are no grounds for sound reasoning upon any state of things different from what we now see. However, we are obliged to form some theory on this question if we would speculate on the constitution of the depths beneath the cooled surface of the globe.

Among those who have written upon the question, Dr Sterry Hunt, of Montreal, has perhaps offered the most plausible explanation. Impressed with the necessity of accounting for the presence of water in the volcanic laboratory, and assuming that all the water belonging to this planet must have existed originally as a gaseous envelope surrounding a still incandescent solid ball, he appears to have seen no means of accounting for this subterraneous water, except by supposing it to have been subsequently derived from the atmosphere by precipitation.

"While admitting with Hopkins and Scrope the existence of a solid nucleus and a solid crust, with an interposed stratum of semi-liquid matter, I consider this last to be, not a portion of the yet unsolidified igneous matter, but a layer of material which was once solid, but is now rendered liquid by the intervention of water under the influence of heat and pressure. When, in process of refrigeration the globe had reached the point imagined by Hopkins, when a solid crust was formed over the shallow molten layer which covered the solid nucleus, the farther cooling and contraction of this crust would result in irregular movements, breaking it up, and causing the extravasation of the yet liquid portions confined beneath. When at length the reduction of temperature permitted the precipitation of water from the dense primæval atmosphere, the whole cooling and disintegrating mass of broken up crust and poured out

igneous rock would become exposed to the action of air and water. In this way the solid nucleus of igneous rock became surrounded with a deep layer of disintegrated and water-impregnated material, the ruins of its former envelope, and the chaotic mass from which, under the influence of heat from below and air and water from above the world of geologic and of human history was to be evolved[1]."

And further on, he explains the relation which this supposed reconstituted and re-heated outer layer holds to his volcanic theory: for "considering the conditions of its formation" he thinks that "water would be necessarily absent from the originally fused globe, in which the older school of geologists conceive volcanic rocks to have their source[2]."

However it may be questioned whether a layer of material simply deposited from water, in a manner similar to that of the ooze or sand of the ocean bed, would after consolidation into rock, and re-heating, supply sufficient water to account for the immense quantities of steam given off during volcanic eruptions. It appears that there ought to be, under this view of its derivation, the same equivalence between the amount of the lava and of the gaseous emanations from a vent, as between the earthy sediment and the water incorporated with it when it became consolidated after precipitation. Although no exact measures are known, still it seems that the water given off in steam bears too large a proportion to the solid matter to be thus accounted for; especially when we remember that, besides the emanations from the crater, a large quantity of steam is emitted by the lava stream after eruption, before it becomes cool.

The presence of so large a quantity of water substance in the deep-seated and molten rocks is still to seek.

The following supposition has been suggested by a paper read by Mallet before the Geological Society[3], although it will

[1] Sterry Hunt's "Lecture on Volcanoes and Earthquakes," p. 6. This short lecture without date or name of publisher, contains a clear résumé of the writer's views.

[2] *Ibid*, p. 8.

[3] "Quart. Journ. Geol. Soc.," vol. xxxvi., p. 112.

be apparent that it is not the same theory as his. It depends upon the undoubted fact, that, if all the water upon the face of the globe were to be at the present moment converted into vapour, the pressure which it would exert at the earth's surface would be the same as would be exerted by the present oceans, were they spread in an equable layer over the surface. Mallet enters into speculations regarding the temperature to which water might be raised under such pressure, and its effect upon rocky matter.

Let us then revert to the far distant time when the temperature of the earth had fallen to that point, when the oxygen and hydrogen under the said pressure were first able to combine. The pressure, caused by the gravitation of the water substance formed from them, would have been that due to a layer of liquid water the exact equivalent of the whole mass of the two gases. This would have been the same as the pressure of a column of an ocean somewhere about two and a half miles deep. The water substance would at that time have been at far above its critical temperature, and possessed of great solvent power upon silicates and some other minerals. It may then be asked, Is it necessary to suppose that the rocky materials had already formed a solid globe, upon which this water substance was supernatant? Might not rather such substances, as were soluble, have been in solution with it, even to that depth at which the magma was succeeded by denser materials not capable of holding water-gas in solution? If that were so, then as cooling proceeded a crust of rock would have been formed by crystallization at a certain level, and would have gradually extended downwards. But there may be even still remaining an intensely hot layer of original water-impregnated silicates, underlying the solid crust, ever in readiness to furnish the steam, gases, and ejectamenta of the volcano.

In our ignorance of the properties of water at the high temperatures and under the pressures we are now considering, it is not possible to arrive at any but the most general conclusions, such as the following.

If we take the mean depth of the ocean to be 2500 fathoms, we find that the pressure due to a layer of water of this depth would

be 442 atmospheres, or 336424 mm. of mercury. But we must reduce this in the proportion of 146 : 197[1], because the depth will be inversely proportional to the area, and we are now supposing the ocean spread over the whole globe. This will make the pressure about 327 atmospheres, or 249329 mm. of mercury. We do not know what temperature corresponds to this pressure. It was found by M. Cagniard de la Tour that the critical temperature of water is about 773° F., but the pressure was not determined. At that temperature the water began to dissolve the glass tube which contained it. At the temperature 230° C., or 446° F. the pressure of steam is 20926 mm. of mercury[2]. This appears to have been the highest determined by Regnault.

It is at once apparent that the temperature corresponding to 249329 mm. of mercury must be greatly above the critical temperature.

Silicates would have begun to be deposited in a solid and probably crystalline state before any rain could fall from the atmosphere, because precipitation of water could not take place until the temperature had decreased to about 773° F. A portion of sea salts would have remained in solution in the primitive ocean after the silicates had crystallized out, another portion would still continue to form a constituent part of the hot magma supposed to exist beneath the solid crust in a state of igneo-aqueous fusion.

This igneo-aqueous fusion has usually been spoken of as a state of solution of the rock in water. It is a condition of which we know little. Probably it may be more correctly described as a state of solution of water in rock in accordance with Henry's law[3]. And some experiments by Mr Hannay throw an interesting light upon the subject. He doubts the conclusions of Andrews as to the continuity of the states of matter, and it would appear from his reasoning, that, when a substance is confined at a temperature above the critical, it is

[1] Ch. IX., p. 112.

[2] Balfour Stewart's "Heat," 2nd ed., p. 402. Locomotive engines are constructed to work up to a pressure of about 10 atmospheres.

[3] See Chap. V.

really in the gaseous state; and that in this state it is capable of holding solids in solution, which in the vaporous state it cannot do[1].

We may therefore look upon the state of igneo-aqueous solution as one in which the water-substance is in a gaseous state, and consider that the combination between the water-substance and the rock is probably of that kind which has been termed "occlusion" of gas by a liquid, as explained in the fifth chapter.

If, as we have shown on geological evidence to be almost certain, there exists a liquid substratum of fused rock, there appears to be an *à priori* probability from the following considerations, that it must contain water dissolved, or "occluded," in it. We know that by far the larger proportion of substances ejected from volcanos, or raised from great depths among plutonic rocks, consists of the bases combined with oxygen. Nevertheless, in the primitive condition of the earth, the temperature must have been too high to allow of this combination taking place, and the elements will have existed in a state of dissociation. Among the heterogeneous substances still gaseous gravitating towards the centre of attraction would have been oxygen and hydrogen, at a very high temperature but still gaseous and uncombined. These would by the law of diffusion have formed a mixture with other gases; they could not as gases have existed in separate layers. In fact it is obvious that at some time or another oxygen has had access to those bases, and has combined with them. It seems then probable that, while the earthy bases took up some of the oxygen to form silica, alumina, lime, magnesia, soda, potash, iron oxide, and the rest; hydrogen would simultaneously take up its share to form water, which we know from its occlusion in lavas would be readily soluble in the magma then being constituted. Thus water would be an original constituent of the magma, as well as what, in this connection, we may call the *other* minerals.

The only alternative supposition appears to be, that the

[1] "Proc. Roy. Soc.," vol. XXXII., p. 410. June 16, 1881. See also *ibid.* p. 407.

oxygen, which forms so enormous a part of rocks, has been abstracted from the ocean, and carried down by imbibition to the profound depths, from which plutonic and volcanic rocks have come up. This leads to Davy's theory of underground temperature. But, besides other objections to it, it may be asked, what has become of the equivalent hydrogen?

CHAPTER XII.

CRUST NOT FLEXIBLE.

Supposed condition of substratum re-stated—Intense corrugation a fact remaining to be explained—Rounded contour of greater corrugation not to be expected—Problem of a flexible layer resting on a liquid solved—General conclusions from the result—These not in accordance with the phenomena—Hence we conclude the crust must crush together and break up—Arrangements of parts which would ensue.

We have thus far arrived at the conception of a highly heated layer of rocky matter combined with water substance, the latter being kept in a state of compression by the superincumbent pressure of the crust. This layer is liquid, and in a certain sense elastic, and is ready to burst forth with the evolution of steam and gas wherever, and whenever, a vent is opened for its escape. We have also, as we believe, proved that such a layer, on account of its expansibility, need not exhibit tides[1]. We may therefore consider ourselves to be freed from the trammels imposed by the hypothesis of a solid globe, contracting from the effects of cooling only.

The substratum may be regarded, when under the normal compression arising from the weight of the crust, as being saturated, and therefore incompressible; *i.e.* an additional weight will not reduce it in volume, although, upon the weight being lightened, it would expand and increase in volume.

[1] Chapter V.

Intense corrugation, although many regions of the earth's surface are comparatively free from it, is nevertheless one of the commonest conditions of the strata. We believe we have proved that the cooling of a solid globe cannot explain this phenomenon. Deferring for the present further speculation as to its cause, we accept the fact; and will now enquire what sort of arrangement the parts of a compressed region of solid crust would take on, supposing it to rest on a liquid substratum of approximately uniform density, being under such pressure, as would normally prevent the dissolved gas from being liberated.

It is not unusual to see geological diagrams, which represent a crust of nearly uniform thickness, resting in undulations upon a liquid substratum, in the manner of the first figure of the diagram on page 85; where the contours of the surfaces of the crust and subjacent liquid are drawn as approximately parallel to each other, and where the anticlinals and synclinals alike present rounded contours. It seems to be imagined, that a tract resting on a liquid substratum, on being laterally compressed, might bulge upwards after the manner of the brick coping of a wall, which from some cause has expanded in length[1].

But it must be remembered that the analogy is not perfect. If the coping of a wall expands it must bulge upwards, because the wall on which it rests prevents it from bulging downwards. If however, a bar of any heavy material was merely held fast at the two ends but not otherwise supported, and then caused to expand, it is certain that it would not bulge upwards, but downwards. The case of a portion of the earth's crust resting on a liquid is not exactly analogous to either of the above instances, but comes nearer to the latter than to the former.

We will now investigate mathematically what form a layer of material, resting on a liquid would assume, if compressed at the two ends. We are obliged to assume it to be perfectly flexible, because the supposition of any degree of rigidity would render the problem incapable of solution, since, in all problems of that kind, the differential equation is not linear, unless the

[1] T. M. Reade, "Effects of Alternation of temperature on Terra Cotta copings etc." "Geol. Mag.," N.S., Dec. III., vol. V., p. 26, 1888.

bending is assumed to be only inchoate, which would not give what we are seeking, namely the form of the curve. But if we find the form for a flexible layer, we may be satisfied that the form would not be so *very* different if it were somewhat rigid, as to render our result useless.

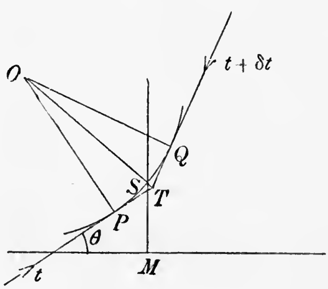
In order to obtain some insight into the general character of the corrugations, we will begin by enquiring what form would be assumed by a heavy flexible crust, resting upon a liquid within a rectangular trough shorter than the crust; for this would give an approximate idea of the contour of the surface upon the course of a section of the sphere perpendicular to its surface and cutting a set of corrugations at right angles.

It may be assumed that the trough and crust were originally of the same length, and that the corrugations have been produced by the ends of the trough having been made to approach each other.

Let the axis of x be horizontal, the axis of y vertical.

θ = the inclination of the tangent at P to the horizon.
r = the radius of curvature at P.
ρ = the density of the crust.
σ = the density of the liquid.
p = the pressure of the liquid at P.
t = the force of compression in the crust in the direction of the tangent at P.
k = the thickness of the crust.

As already stated, we suppose the crust perfectly flexible.



Let PQ be an element of the curve, PT and QT, tangents at its extremities. O the point which, when P and Q coincide,

becomes the centre of curvature. t is a compressing force at P acting in PT, $t+dt$ is the compressing force at Q. The pressure p of the liquid acts in the direction TS, and the weight of the element in SM.

Hence for equilibrium we must have, resolving the forces along SO,

$$pPQ - g\rho k\, PQ \cos\theta - t\frac{PT}{OT} - (t+dt)\frac{QT}{OT} = 0.$$

In the limit, $PT = QT = \frac{1}{2}PQ$; and $OT = r$. Therefore dividing by PQ, we have in the limit,

$$p - g\rho k \cos\theta - \frac{t}{r} = 0 \qquad (1).$$

Resolving the forces perpendicularly to SO;

$$t\frac{OP}{OT} - (t+dt)\frac{OQ}{OT} - g\rho k\, PQ \sin\theta = 0.$$

Divide by PQ, and proceed to the limit, observing that OP, OQ, and OT, are then equal,

$$\therefore \quad \frac{dt}{ds} + g\rho k \sin\theta = 0.$$

$$\therefore \quad \frac{dt}{ds} = -g\rho k\frac{dy}{ds}.$$

$$\therefore \quad t = g\rho k\,(C-y) \qquad (2).$$

To find the value of C we may proceed thus.

It is evident that, either at an anticlinal or a synclinal, the compression is the same on both sides of it. So we shall have for equilibrium, taking ds as the element, at the anticlinal or synclinal, and t the compressing force,

$$2t\sin\theta \mp g\rho k ds \pm p ds = 0.$$

When ds is indefinitely diminished, t becomes the compression, at the anticlinal, or synclinal; and we have,

$$t\sin\theta = 0.$$

$$\therefore \quad \text{either } t = 0, \text{ or } \sin\theta = 0.$$

But by (1) we have generally,

$$p = g\rho k\cos\theta + \frac{t}{r}.$$

First suppose that $t = 0$, and $\sin\theta$ not $= 0$.

Then $$p = g\rho k \cos\theta \text{......................(A).}$$

Secondly suppose that $\sin\theta = 0$, and t not $= 0$.

Then $$p = g\rho k + \frac{t}{r} \text{......................(B).}$$

It is evident that (A), being the smaller value, must give the fluid pressure at the anticlinal; while (B), being the larger, must give that at the synclinal: whence we gather, that at an anticlinal

$$t = 0, \text{ and } \cos\theta = \frac{p}{g\rho k}.$$

The corresponding double value for θ indicates a cusp-like form at the anticlinal.

Hence, if h be the height of the anticlinal above the axis of x, we get from (2),

$$0 = C - h, \quad \therefore\ C = h.$$

And $$t = g\rho k\,(h - y).$$

Now, the fluid pressure = the pressure due to the depth below the anticlinal + the pressure at the anticlinal.

Let us call the latter $g\rho\delta$.

Then, $$p = g\sigma\,(h - y) + g\rho\delta.$$

Equating values of p;

$$g\sigma\,(h - y) + g\rho\delta = g\rho k \cos\theta + \frac{t}{r},$$

$$= g\rho k\left(\frac{dx}{ds} + \frac{h - y}{r}\right),$$

$$= g\rho k\left\{\frac{dx}{ds} - (h - y)\frac{d}{dy}\left(\frac{dx}{ds}\right)\right\}.$$

Change the origin to the level of the anticlinals by writing z for $h - y$, and the equation becomes,

$$g\sigma z + g\rho\delta = g\rho k\left\{\frac{dx}{ds} + z\frac{d}{dz}\left(\frac{dx}{ds}\right)\right\},$$

$$= g\rho k\frac{d}{dz}\left(z\frac{dx}{ds}\right).$$

Therefore integrating,

$$g\sigma\frac{z^2}{2}+g\rho\delta z=g\rho kz\frac{dx}{ds}+D.$$

At the level of the anticlinals $z=0$, and $\dfrac{dx}{ds}$ is finite,

$$\therefore\ D=0.$$

Hence $\qquad z=0$, is one solution of the problem ;
or the crust will rest in equilibrium, when lying horizontally upon the liquid, as is self evident.

Dividing by gz, and putting

$$1+\left(\frac{dz}{dx}\right)^2 \text{ for } \left(\frac{ds}{dx}\right)^2, \text{ we get}$$

$$\frac{dx}{dz}=\frac{\dfrac{\sigma z}{2}+\rho\delta}{\sqrt{(\rho k)^2-\left(\dfrac{\sigma z}{2}+\rho\delta\right)^2}}.$$

Integrating, and putting the constant $=0$, we obtain finally,

$$x^2+\left(z+\frac{2\rho}{\sigma}\delta\right)^2=4\left(\frac{\rho}{\sigma}\right)^2k^2.$$

This represents a circle, whose radius is $\dfrac{2\rho}{\sigma}k$, and whose centre, recollecting that z is measured downwards, is at the height $\dfrac{2\rho}{\sigma}\delta$ above the anticlinals.

If λ be the distance from one anticlinal to the next,

$$\lambda=\frac{4\rho}{\sigma}\sqrt{k^2-\delta^2},$$

whence $$\delta=\sqrt{k^2-\left(\frac{\sigma\lambda}{4\rho}\right)^2}.$$

This gives $g\rho\delta$, or the fluid pressure at the anticlinal, in terms of the thickness of the crust and the distance between two anticlinals.

These conditions are possible so long as λ is less than $\dfrac{4\rho}{\sigma}k$; when it has that value, the festoon between the anticlinals becomes a semicircle.

It appears therefore that, when no extraneous force acts upon the crust, a section of it will assume the form of a series of equal circular arcs, arranged in festoons, the radii of which depend solely upon the mass (ρk) of a unit of length of the crust, and upon the density σ of the liquid on which it is supported.

The next step is to conceive a long trough, bent lengthwise into a circular form of large radius, and that the crust and liquid are acted upon by a force gravitating towards the centre, and so we approximate to what would be, in the case we have supposed, a section of the earth's surface, taken along a great circle. In this case we must suppose the curve reentering. No extraneous force acting, the circular festooned form will approximately represent the curve of equilibrium.

But for simplicity of conception it will be better to return to the idea of the rectangular trough, and to suppose that the curve is in the phase of an anticlinal where it meets the trough at either end; which condition is necessary for equilibrium if there is no friction there: and that there are m festoons.

Having found the form of equilibrium we have now to enquire what relation it holds to the length of the trough.

We will consider that the trough was originally of the length L, and that it is compressed until its length becomes $L(1-\epsilon)$. Then since $\frac{2\rho}{\sigma}k$ is the radius of every circular arc which can admit of equilibrium, and that the chords of the m festoons, or arcs, must equal the reduced length of the trough, putting ϕ for the angle subtended by each festoon, we must have

$$m \,.\, 2 \,.\, \frac{2\rho}{\sigma} k \sin\frac{\phi}{2} = L(1-\epsilon),$$

and because the whole length of the crust is that of the trough before compression,

$$\therefore \quad m\frac{2\rho}{\sigma} k\phi = L;$$

whence $$\frac{\sin\frac{\phi}{2}}{\frac{\phi}{2}} = 1 - \epsilon \quad\text{..........................(1)},$$

and $$m = \frac{\sigma}{2\rho}\frac{L}{k\phi} \quad\text{..........................(2)}.$$

Any value of ϵ less than unity substituted in (1) will give a corresponding value of ϕ, and thence from (2) a value of m. But none except integral values of m will be compatible with equilibrium, because the curve must meet either end of the trough at an anticlinal.

ϕ diminishes as m increases, and when m is infinitely great and ϕ infinitely small

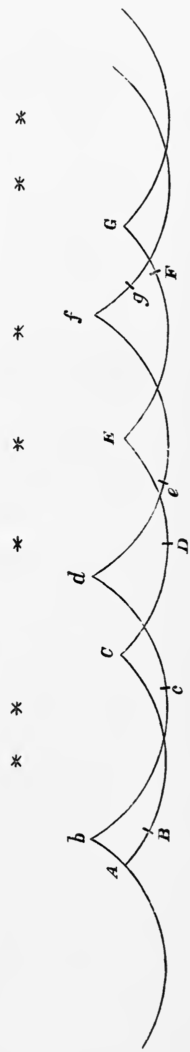
$$\frac{\sin\frac{\phi}{2}}{\frac{\phi}{2}} = 1 - \epsilon$$

becomes unity, and therefore $\epsilon = 0$, or there is no compression.

m also increases as k diminishes. Hence, *cæteris paribus*, the festoons are more numerous with a thin crust than with a thick one. The geometrical relations show that the lengths of the festoons must increase, and their number diminish, as the compression is increased.

Consider now the crust to be in equilibrium with a certain pair of values of m and ϵ, m being integral, and let the trough then be shortened, but not sufficiently so for the next integral value of m to be reached. What will happen? The festoons cannot alter their curvature so as to adapt themselves to their lessened chords. It seems then that the compression at the anticlinals must become finite, instead of being zero, and that the ends of the festoons will be pushed up against each other, by which means material will be accumulated there. This will accord with an undulating form of the curve, but it seems that the equilibrium must become unstable, on account of the weight resting on the anticlinal in the highest possible position. It will therefore be liable to slide down upon the surface of the liquid on one side or other of the anticlinal.

If the conditions of perfect fluidity in the liquid and perfect flexibility in the crust were fulfilled, and the crust were of insensible thickness, then our result would be true, and the festoons be all equal circular arcs. But under such circumstances as might be supposed to exist in nature, these conditions would be so imperfectly fulfilled, that all we can assert is that the above considerations will serve as a rough guide to what we might expect to occur. The conditions of equilibrium would be satisfied from point to point not very widely distant from each other, and we might expect the compression to be distributed unequally. Hence the festoons would be larger, and the anticlinals higher, in some regions than in others. The local thickenings of the crust would also render the forms of the curve locally to differ from the normal form. The effect of the hydrostatic pressure of the oceans would also have to be taken into account.



The diagram is intended to illustrate which might be expected to be the effects of increased compression, in elevating or depressing points in the crust. The asterisks are the centres of the circular arcs. The point A is supposed for simplicity to remain fixed. Then the points B, C, D, E, F, G, upon further compression assume the positions b, c, d, e, f, g.

If we now pass to the case of any rectangular vessel, and suppose it to be compressed in two orthogonal directions, then the form of the corrugations in these two directions would be approximately governed by the laws we have investigated, provided the plane crust be supposed capable of stretching to adapt itself to a form not developable into a plane. And even in the case of a contracting sphere, the *character* of the corrugations may be

assumed to be on the whole similar to that investigated, and they would be arranged about polygonal areas.

The consequence of the crust not being absolutely flexible would be, that it would rest within certain limits in forms either more or less curved than the proper surface of equilibrium, and it is obvious that the considerable ratio, which the thickness of the crust may bear to the radius of the festoon, renders the result of the investigation much more difficult of application to the case of nature than it would otherwise be. But we may use it for what it is worth.

Still further, the general compression along a great circle will very inadequately represent what would occur when the compression over the whole surface has to be taken into account. In short, the case of nature is so extremely complicated, that it is very difficult to reason satisfactorily upon it. Nevertheless the conclusions we have arrived at concerning the case of a thin heavy flexible crust resting on a perfect liquid have a certain value as far as the results may be summed up in the following propositions:

(1) A vertical section of the lower surface of the crust, where it meets the liquid, when carried across a series of the corrugations, would present a series of festoon-like arcs approximating more or less to a circular form, concave upwards.

(2) The anticlinals would be of cusp-like form, resulting from the intersection of contiguous festoons. There would not be found anything of the character of rolling undulations with flat-crested anticlinals.

(3) The degree of curvature of the festoons would not depend upon the amount of the general compression, though their amplitude, or the distance from crest to crest, would be increased, under circumstances of equilibrium, (*i.e.* where m, p. 159, is integral), upon the compression being increased. The curvature would diminish upon the thickness being increased because $r = 2\dfrac{\rho}{\sigma}k$.

(4) But the curve of equilibrium, even if it were once exactly established, could not be strictly maintained during

successive compressions, because we have seen that, in the case of a trough of given length, a flexible crust, and a perfect liquid, equilibrium can only subsist for certain special amounts of contraction having relation to the length of the trough. No doubt the limits within which this would be possible would be enlarged by defects in flexibility and liquidity, and they would also be enlarged by a great increase in the length of the trough as compared to the thickness of the crust: for then m would always be nearly an integer.

It can scarcely admit of a doubt that the results we have just obtained bear no similarity to the case of nature. There is no reason to believe that the crust of the earth is flexed on the whole in such a manner, that a section carried across a series of anticlinals would give a series of circular arcs of the dimensions required by our results; and accordingly our assumptions cannot be in accordance with the facts. The radius $\frac{2\rho}{\sigma}k$, σ being necessarily greater than ρ, is too small to suit the arrangement of the chains.

Now a negative conclusion of this kind is far from being without importance; for it is by a method of exhaustion that we are best able to attack our problem. What then are the assumptions upon which we have arrived at this unsatisfactory result? They are, that the crust is (1) thin, and (2) flexible, and (3) that it rests on a liquid substratum. Of these assumptions the first cannot be wrong. The crust is no doubt thin as compared with the other dimensions with which we have to deal; as, for instance, the width of a continent. That it rests on a liquid substratum has been already shown from geological evidence to be almost certain. It remains then that the error in our assumptions lies in supposing the crust so flexible as not to break up under the action of compressing force.

It follows that, when a horizontal pressure acts upon a tract of crust, we must not suppose that the yielding will take place merely by the bending of the crust, while its uniform thickness is maintained; for, if that were the case, an arrangement of anticlinals, bearing a close resemblance to the result of our investigation, would be produced. But we must rather suppose

that the yielding takes place through a crushing together and thickening of the crust.

The consequence will be that elevations above will be accompanied by depressions beneath. The anticlinals will not be filled with liquid from below, but will be the upper portions of double bulges, which will dip into the liquid below as well as rise into the air above. The arch-like folds, which are often met with, will in that case not affect the entire thickness of the crust; so that they will not be accompanied by anticlinal crests in the liquid beneath them. They will then indeed bear some analogy to the coping of a wall, lifted by expansion from its bedding, because they will have risen from off a solid support. And this is the condition of things upon which we shall attempt to reason in the following chapter.

CHAPTER XIII.

DISTURBED TRACT.

Conditions of equilibrium of a disturbed tract of the earth's crust—Illustrated—Mean levels—Elevations and depressions referred to them—Effects of compression—Work of compression distributed partly against gravity, partly against friction—Probable effects upon a portion of crust—M. Tresca's experiments—Part of the crust sheared upwards and part downwards—These separated by a "neutral zone"—Calculation of its position—Confused contortions in highly metamorphosed rocks explained—Depth of neutral zone—Probable thickness of crust—Prof. A. Favre's experiments on contortions—Inverted flexures—Formation of a mountain chain and its elevation above the ocean—Subsequent effects of denudation and accumulation of additional sediment—Consequent tilting of tract—Also general elevation from the same cause—Why areas of deposition are sinking areas—Degree of tilting depends upon width of tract and the existence of a mountain chain—Basaltic overflows.

In this chapter we shall consider the process of elevation of a mountain chain by horizontal compression under the condition of a certain degree of rigidity in the crust, without making any particular assumption regarding the cause of compression, assuming only that it acts in a horizontal direction, and that the crust rests on a liquid substratum of greater density than its own. Under these circumstances, if a hole were made anywhere in the crust, the liquid would rise in it to a certain level, which we may call the effective level of the liquid.

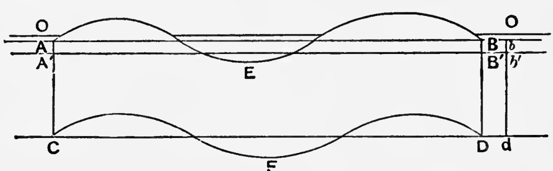
The case we are about to consider may then be illustrated in the following manner. Suppose there to be a pond of water with a quantity of logs of wood floating in it. The surface

may be entirely covered with them, and they may lie in places with several layers heaped on one another, and the logs may consist of woods of different densities. Or instead of the logs, we may conceive a coating of ice of varying thickness. Then if the surface of the water is at any place exposed to view, it will be found to stand at a certain height above the bottom of the pond. This will mark the effective level of the liquid. Now so long as neither logs nor water are added or removed, you cannot alter the effective level. You may push the logs about into fresh positions, heap them one on another, or in the other case supposed, you may break up the ice and heap it together, thicken it in some places, and thin it in others: the effective level of the liquid remains all the while where it was at first.

This analogy will hold for the earth's crust resting in equilibrium on a liquid substratum, so long as we leave contraction of the volume of the globe out of account.

Let us as before call the density of the crust ρ, and that of the liquid σ, σ being greater than ρ. And let our sections be in all cases supposed to be of unit of width.

Premising that the subjoined diagram is drawn for the sake



of convenience, as a mere general representation of a compressed tract of crust, although in the preceding chapter we have expressly proved that it is not this form of contortion which would be likely to be produced, let $A'B'b'$ be the effective surface of the liquid, the crust beyond AC and BD being undisturbed by compression. Then by the principles of hydrostatics

$$B'D = \frac{\rho}{\sigma} k,$$

and $$BB' = \frac{\sigma - \rho}{\sigma} k.$$

But if we suppose an ocean OO, of depth δ and density μ, to cover the crust, we must add the pressure of the ocean to that of the crust and then

$$B'D = \frac{\rho k + \mu\delta}{\sigma},$$

and
$$BB' = \frac{\sigma - \rho}{\sigma} k - \frac{\mu}{\sigma} \delta.$$

To avoid circumlocution, we shall in future call the portion of the crust AB, which has been either partially or generally compressed, and disturbed from its original position between AC and BD, "the tract."

As has been already explained in Chapter VII.[1], if we know the original levels of the upper and under surfaces of a tract, then the volumes of the elevations and depressions of the surfaces with regard to those levels afford a practicable measure of the compression, to which the tract has been subjected. But, because we now pass to the case of a tract of limited extent, we shall make a fresh supposition about the levels, above and below which the elevations $\Sigma(a)$ &c. are measured; no longer regarding them as what would have been the upper and under surface of the crust if it had been perfectly compressible in a horizontal direction; but now, as "mean levels," above and below which the tract is disturbed as compared with uncompressed regions. The consequence of this alteration will be, that when a volume β is depressed into the liquid, it will not follow that an equal volume α will rise into the anticlinals, because the positions of the mean levels themselves will be affected. Hence $\Sigma(\alpha)$ will not be equal to $\Sigma(\beta)$, and we cannot say that $\Sigma(a) - \Sigma(b)$ is equal to klc.

Supposing no attachment at AC and BD, if there be equilibrium of the whole as a rigid mass, we must have two conditions fulfilled, (1) that the weight of the fluid displaced equals the weight of the whole mass, and (2) that the centre of gravity of the whole mass is vertically above that of the fluid displaced.

Let us consider the former condition first. Then, using the

[1] p. 85, *et seq.*

same letters, but dashed, to refer to the effective surface of the fluid, as we have used for the upper surface of the crust, p. 85, and supposing that a volume $\Sigma(d)$ of water covers the hollows of the disturbed region, since the weight of the fluid displaced is

$$g\sigma A'B'DFC, \text{ or } g\sigma\{kl(1+c)-\Sigma(a')+\Sigma(b')\},$$

and the weight of the mass supported is

$$g\rho kl(1+c)+g\mu\Sigma(d),$$

we have for the first condition of equilibrium,

$$\sigma\{kl(1+c)-\Sigma(a')+\Sigma(b')\}=\rho kl(1+c)+\mu\Sigma(d).$$

$$\therefore \Sigma(a')-\Sigma(b')=\frac{\sigma-\rho}{\sigma}kl(1+c)-\frac{\mu}{\sigma}\Sigma(d).$$

But we also have the geometrical relation,

$$\begin{aligned}\Sigma(a')-\Sigma(a)+\Sigma(b)-\Sigma(b') &= \text{rectangle } AA'B'B,\\ &= BB'\times l,\\ &= \frac{\sigma-\rho}{\sigma}kl-\frac{\mu}{\sigma}\delta l;\end{aligned}$$

therefore subtracting,

$$\Sigma(a)-\Sigma(b)=\frac{\sigma-\rho}{\sigma}klc+\frac{\mu}{\sigma}\{\delta l-\Sigma(d)\}\text{.........}(1)$$

Since

$$\Sigma(a)-\Sigma(b)+\Sigma(\beta)-\Sigma(\alpha)=klc\text{[1]};$$

$$\therefore \Sigma(\beta)-\Sigma(\alpha)=klc-\frac{\sigma-\rho}{\sigma}klc+\frac{\mu}{\sigma}\Sigma(d)-\frac{\mu}{\sigma}\delta l,$$

or

$$\Sigma(\beta)-\Sigma(\alpha)=\frac{\rho}{\sigma}klc-\frac{\mu}{\sigma}\{\delta l-\Sigma(d)\}\text{..........}(2),$$

in which expression $\delta l-\Sigma(d)$ is evidently the volume of the water which has been displaced from the elevated part of the crust by its being elevated. If the quantity of water so displaced is small, or if we are dealing with a tract that was land before the movement took place, then

$$\frac{\Sigma(\beta)-\Sigma(\alpha)}{\Sigma(a)-\Sigma(b)}=\frac{\frac{\rho}{\sigma}klc}{\frac{\sigma-\rho}{\sigma}klc}=\frac{\rho}{\sigma-\rho}.$$

[1] Equation (1), p. 85.

And consequently, if σ were infinite we should have

$$\Sigma(\beta) - \Sigma(\alpha) = 0,$$

as would be the case, for under such circumstances the crust could not dip down into the fluid, nor the fluid rise into anticlinals.

The above ratio, which does not contain the coefficient of compression, will be true under all circumstances for a continental tract resting in hydrostatic equilibrium on a denser substratum. If we suppose the crust to be of the density of granite, viz. 2·68, and the subjacent fluid of the density of basalt, viz. 2·96[1], we have,

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta) - \Sigma(\alpha)} = \frac{\sigma - \rho}{\rho} = 0{\cdot}104,$$

or about one tenth[2]. The diminution of density by elevation of temperature need not be regarded.

We have thus far had under consideration the relations of the elevations and depressions above and below the mean levels, as they would depend upon the conditions of equilibrium of a mass of any form, floating without external constraint upon a fluid, so far as the vertical forces are concerned. We must however also consider how much importance belongs to the attachments of the tract at its extremities AC and BD; and on this account, and from our general ignorance of the amount of

[1] Calculating from Laplace's law of density,

$$\frac{d\rho}{dr} \text{ at the surface} = 0{\cdot}00000053,$$

r being expressed in feet. Consequently the increase to ρ in 20 miles would be 0·056 and the density of basalt would be met with at 100 miles. But Laplace's law is empirical, and does not give information as to the actual arrangement of the successive couches.

[2] The following memoranda will be useful. According to the hypothesis,

$$\sigma = 2{\cdot}96, \qquad \rho = 2{\cdot}68, \qquad \sigma - \rho = 0{\cdot}28,$$

$$\frac{\rho}{\sigma} = 0{\cdot}905, \qquad \frac{\sigma}{\rho} = 1{\cdot}104,$$

$$\frac{\sigma - \rho}{\rho} = 0{\cdot}104, \qquad \frac{\sigma - \rho}{\sigma} = 0{\cdot}095,$$

$$\frac{\rho}{\sigma - \rho} = 9{\cdot}57, \qquad \frac{\sigma}{\sigma - \rho} = 10{\cdot}57.$$

rigidity which must be attributed to the mass, it is obvious that we can only arrive at the most general conclusions.

Let us then suppose as before, that the portion of crust $AbdC$ is compressed into the position $ABDC$. The lateral pressure will act in some straight line drawn from a point in AC to a corresponding point in BD. Consequently it will have no direct tendency to open any fissures in the crust. We may therefore dismiss from consideration such a condition of things as is represented in our second figure of the diagram on page 85.

If the crust were flexible and elastic, such a form as that represented in the first figure, or on page 165, might be possible. But the condition more in accordance with natural appearances is that it is partially plastic and partially brittle. It is capable of withstanding considerable pressure until it gives way, but when it does so, the harder portions will break up, and the more plastic portions will accommodate themselves among the interstices of the harder.

We will now endeavour to form some idea of the manner in which the work of compression will be distributed between the two effects of increasing the height and increasing the length of the tract affected by the disturbance.

The work done by the compressing force in raising a mountain chain (which we use as a generic term for any elevated tract) will consist of two parts, (1) the work producing deformation of the crust, which will include the work of distortion and cleavage, and is done against the molecular forces, and (2) the work done against gravity. This last will in itself consist of two parts, viz. the elevation of the mass of the mountain above the upper mean level of the crust, and the depression of its root into the fluid, below the lower mean level.

If we take $y = f(l)$ to define the contour of the elevated mass along a section across the tract, the work against gravity on a section of unit of width in raising it to the height y will be

$$g\rho \int \frac{y^2}{2} dl.$$

And similarly, if y' be the ordinate corresponding to y for

the root of the mountain below the lower mean level, the wcrk against gravity in depressing the root will be

$$g(\sigma-\rho)\int\frac{y'^2}{2}\,dl.$$

But
$$y'=\frac{\rho}{\sigma-\rho}\,y.$$

Hence the whole work against gravity, which is the sum of these, will be

$$g\left\{\rho+(\sigma-\rho)\frac{\rho^2}{(\sigma-\rho)^2}\right\}\int\frac{y^2}{2}dl,$$

or
$$g\rho\frac{\sigma}{\sigma-\rho}\int\frac{y^2}{2}dl.$$

If h be the mean height of the elevated tract, then

$$\int\frac{y^2}{2}dl=hl\bar{y},$$

where $\bar{y}$ is the height of the centre of gravity of the tract; so that the work against gravity is

$$g\rho\frac{\sigma}{\sigma-\rho}hl\bar{y}.$$

If now we take λ for the distance through which a compressing force P has acted, while thickening the crust throughout a length l, to raise the range of mean height h and simultaneously to depress its root, we have by equality of volumes,

$$k\lambda=\frac{\sigma}{\sigma-\rho}\,hl.$$

Whence hl is constant. And the work against gravity may be expressed as

$$g\rho k\lambda\bar{y}.$$

For a given amount of compression (λ), this varies as $\bar{y}$. Hence the amount of compressive work necessary, so far as gravity alone is concerned, to shorten a tract through the space λ will diminish, in proportion as the height of the centre of gravity of the tract is diminished, and since hl is constant, it will be least for a given length of tract when its height is uniform; and in that case the work would vary inversely as the length.

Compression therefore, as far as gravity is concerned, would be most readily satisfied by a very long tract being very slightly raised.

But, in estimating the work that has to be done by P in working through the space λ, we must consider not only what is spent on raising the crust against gravity, but also what is spent both on shearing to thicken it, and possibly also on pushing it over the substratum on which it rests.

If there were no friction between the crust and the substratum, the whole work $P\lambda$ might be expended upon a tract of length l situated at any distance off in the line of P's action, and obviously the weakest place would be the first to give way, wherever that might be. But if there is friction on the substratum, part of the work will be expended on pushing the crust along over it, and consequently the work left available for shearing the crust into its thickened form, and also for overcoming gravity, will diminish as the distance from the place of application of the compressing force increases. It follows that the elevation produced will diminish accordingly, and therefore a disturbed tract of an average rigidity will be highest on the side from which the compressing force comes. We see likewise that even a weak place in the crust may escape compression, so long as no compressing force is acting sufficiently near to it. But if there be, the crust will give way there, and a weakened condition is likely to be propagated from that place, and the disintegrating process to accompany it. But it still remains that any friction between the crust and the substratum would tend to localise the effects of a force, whether compressive or extensive, and to confine them to the neighbourhood of the place where the force was applied.

It appears therefore that the length of crust, able to be broken up to give the necessary relief, will depend partly upon the strength of the crust, and partly upon the resistance of friction upon the substratum to the shearing of the crust over it: and that the disturbed length will be greater or less according as the crust is weaker or stronger. But it will be less or greater according as the resistance of the substratum is greater or less.

The horizontal force causing a compression, which, as already remarked, will be relieved rather by crushing or corrugating the rocks than by flexing the crust as a whole, will not as a primary consequence produce the hollows belonging to $\Sigma(b)$ and $\Sigma(\alpha)$. It will rather result in a swelling of the crust upwards and downwards.

The experiments which M. Tresca made upon the flowing of plastic, and even very hard, substances have a bearing upon this subject. His paper has the appropriately paradoxical title "De l'écoulement des corps solides[1]." In it he showed that solid, ductile, soft, or pulverulent, bodies can, without changing their state, flow in a manner analogous to that of liquids, when sufficiently great pressure is exerted on their surface. The substances were subjected to pressure confined in a rigid cylindrical envelope, pierced at the bottom with a concentric orifice of varied dimensions. These experiments would lead us to expect that the materials of the earth's crust, when laterally compressed by a sufficient force, would "flow," or be strained, vertically. What would be analogous to the stream lines in a liquid would represent the distortions of the rocks; thus might be produced cleavage, foliation, or contortion, according to the conditions of the rocks themselves, or of the neighbouring masses which adjoined them[2].

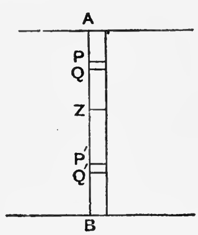
But although we cannot predict the exact manner in which the material will be arranged which constitutes the protuberances that will be produced above and below the mean levels of the crust, yet we may arrive at some conclusion as to whether the material at any given level within the crust

[1] "Comptes rendus," 1865, vol. LX., p. 1228.

[2] A writer under the initials E. L. G. has said that, if the earth were of steel, at the depth of one or two leagues it would be of flowing steel, and that we should have to decide whether flowing steel was solid or fluid ("Eng. Mechanic," vol. XII., p. 254). But in M. Tresca's experiments, one surface of the substance was subjected to enormous pressure, while the orifice from which it flowed was subject to none at all. This is quite a different case from that of the interior of the earth, where the pressure is alike in all directions. Mr W. Hallock describes experiments to show that pressure cannot truly liquefy a solid, *i.e.* diminish its rigidity, so that neither chemical nor mineralogical changes can be produced by pressure alone without a rise of temperature, "American Journal of Science," Oct. 1887, quoted in "Nature," Nov. 10, 1887, vol. XXXVII., p. 47.

will on the average be sheared upwards or downwards by the compression. For this compression, acting horizontally, will have no tendency in itself to determine whether the motion which it causes in an elementary portion of the crust subjected to its action shall be upwards or downwards. That circumstance will therefore be determined by the resistances which operate to influence the motion of the element in question. These will be the weight of the element acting downwards, the friction tending to resist the motion, and opposite to it in direction, and the pressure of the crust upon the liquid beneath. It is evident that the friction among the particles of the crust will depend upon its physical state. If it were rigid it would be infinite; if liquid it would be zero. As it is, we may conclude that its plasticity is increased in the lower parts by the influence of the high temperature there. We will therefore assume the friction to be some function of the depth. It will be sufficient for our purpose to consider the density constant throughout the thickness of the crust.

Let then $AB = k$ be the thickness of the crust before motion commences. When the lateral pressure has become so great



that compression begins to take place, suppose that the portion AZ of the column AB is sheared upwards, and the portion ZB downwards, and let us call the locus of the point Z the "neutral zone."

Let p be the force at P which resists the upward motion of the element PQ;

$$AP = x, \quad PQ = dx,$$

$f =$ the frictional force, which we assume to be a function of x.

Then $$dp = g\rho dx + fdx;$$

$$\therefore\ p = g\rho x + \int fdx + C.$$

At A, $x = 0$; and f has some constant value; and there is no extrinsic pressure at A;

$$\therefore\quad p = g\rho x + \int_0^x fdx \text{.....................(1).}$$

Again, if p' be the force at P' which resists the downward motion of ZP',

$$dp' = -g\rho dx + fdx;$$

$$\therefore\quad p' = -g\rho x + \int fdx + C' \text{...............(2).}$$

Now, at the neutral zone, the forces which resist the upward motion must be equal to those which resist the downward motion. If then h be the depth of the zone, the integral (1), taken from 0 to h, must be equal to (2) taken from h to k, when to the second we have added the extrinsic force $g\rho k$, arising from the upward pressure of the liquid;

$$\therefore\quad g\rho h + \int_0^h fdx = -g\rho\,(k-h) + \int_h^k fdx + g\rho k;$$

$$\therefore\ \int_0^h fdx = \int_h^k fdx.$$

This result shows that the position of the neutral zone depends solely upon the condition, that the friction upon the column above it shall be equal to that upon the column below it.

Let us first of all make the assumption that the friction is constant at all depths, and equal to γ.

Then $$\gamma h = \gamma\,(k-h);$$

$$\therefore\quad h = \frac{k}{2},$$

or the neutral zone is situated in the middle region of the crust.

But if we consider the plasticity of the material to be increased by heat in the lower parts of the crust, we must assume some function to express the value of f which has a constant value γ at the surface, and decreases, at first slowly, and afterwards more rapidly, as the depth x is increased.

For the purpose in hand we may admit that it becomes zero at the bottom of the crust, although this is of course not strictly true. An appropriate function for our purpose appears to be,

$$\gamma \cos \frac{\pi x}{2k}.$$

The corresponding force upon an element at P will therefore be $\gamma \cos \frac{\pi x}{2k} dx$; and the whole frictional force upon AZ will be

$$\int_0^h \gamma \cos \frac{\pi x}{2k} dx,$$

or,

$$\gamma \frac{2k}{\pi} \sin \frac{\pi h}{2k}.$$

If we make $h = k$, we find the friction across the whole crust to be $\gamma \frac{2k}{\pi}$. And it is evident that the friction along AZ, *plus* the friction along ZB, make up the whole friction across AB.

Hence the friction along $ZB = \gamma \frac{2k}{\pi}\left(1 - \sin \frac{\pi h}{2k}\right)$.

Our condition for the position of the neutral zone then gives us,

$$\gamma \frac{2k}{\pi} \sin \frac{\pi h}{2k} = \gamma \frac{2k}{\pi}\left(1 - \sin \frac{\pi h}{2k}\right),$$

$$\text{or} \quad \sin \frac{\pi h}{2k} = \frac{1}{2},$$

$$\therefore \quad \frac{\pi h}{2k} = \frac{1}{3}\frac{\pi}{2};$$

$$\therefore \quad h = \frac{1}{3} k.$$

In this case then the neutral zone would be at one-third of the depth.

In finding the position of the neutral zone no assumption has been made respecting the value of k. So long as the crust at the place under consideration was not under constraint, but in equilibrium as a floating body, the neutral zone would hold the same relative position within it. So that, if this condition of flotation remained fulfilled, it would be always horizontal. For the purpose we have in view it will be sufficiently accurate to assume that it occupies a position whose distances from the upper and lower surfaces of the crust, whether before or after compression, are always in the same ratio.

It is also observable that the position of this zone does not depend upon the absolute value of the frictional force, so long as it can be expressed as a function of the depth multiplied by the value which the friction has at the surface. If however the friction were to vanish, which it would do if the material were to become liquid, we should have h indeterminate. The signification of this appears to be, that the tendency of the material at every depth would then be indifferently to move upwards or downwards, and therefore it would be affected with confused internal currents, somewhat similar to convection currents.

Now it will be observed that, since the shear, whether upwards or downwards, increases continually as we get further from the neutral zone, it follows that if there were to be a relative motion, in the opposite direction to that which they actually have, impressed upon the particles at any given level, that level would become a neutral zone relatively to the particles above or below it. If then the material above or below any zone were to become liquid, it would behave under this relation as the whole crust would behave if it were liquid, and would be affected with confused currents. This consideration goes some way to explain the confused contortions constantly observable in highly metamorphosed rocks; which possibly have been softened by heat, and consequently have approached the condition of a liquid. We may however grant the attributes of approximate liquidity even to apparently rigid rocks, when the distorting stress has acted on them continuously for a long time.

The function $\gamma(\cos \pi x/2k)$, which we have provisionally adopted to express the friction at the depth x, would actually vanish at the bottom of the crust. This of course represents what could not really happen. It is however evident, that any softening of the lower parts would tend to raise the neutral zone to a higher position, and that the more complete the softening the higher it would be. We have here assumed an amount of softening which is too great, and find that the corresponding position of the neutral zone would be at the depth of one-third of the crust. On the other hand we have found that, if the crust were equally rigid throughout, it would be situated at the depth of one-half of the thickness. Comparing the two cases, we may fairly conclude that it will be in fact in a position intermediate between these two, and shall be probably justified in placing it at about two-fifths of the thickness.

On different, and geological, grounds we have likewise the following considerations to help to guide us in affixing a limit below which we must place the neutral zone. It is well known that granite frequently occupies the central axis of mountain chains. Now Mr Sorby has taught us in his sectional address at the British Association, 1880[1], that granite has been usually formed in the presence of liquid water; although he speaks of a single instance in which the temperature at the commencement of crystallization may have been higher than that at which water can exist in the state of a liquid. The critical temperature for water is about 773° F. At the rate of increase of one degree F. for 51 feet, this temperature would be reached at the depth of about seven miles and at the rate of one degree for 60 feet at the depth of about eight and a half miles. We may therefore conclude, that rock, which was once at seven or eight miles' depth, has been forced upwards, and consequently, that the neutral zone is lower than that.

But we know of no rocks at the surface except the truly eruptive, which have come from a greater depth than has granite. Had it come from the neutral zone, this would have made $\frac{2}{5}k$ equal to from 7 to $8\frac{1}{2}$ miles, and therefore k would have been from 17 to 21 miles. But since it is very improbable that

[1] "Nature," vol. XXII., p. 392.

any portion of the neutral zone should be brought so near the surface as to be exposed, we may conclude that the crust is thicker than when thus estimated.

Again, Mr Mallet has determined the temperature of melting slag to be about 3000° F.[1], while Sir I. Lowthian Bell gives 1500° C., which very closely agrees[2], and we can hardly suppose that the materials of the crust could be solid at such a temperature, as can melt silicates without the presence of water. This temperature would, at the same rates of increase, be found at the depths of about 28 or 30 miles, consequently we may conclude from this argument that the crust is certainly less than 30 miles thick. One of these reasons then leads us to think that it is more than 17 or 21, and the other that it is less than 28 or 30 miles in thickness, according as we assume the higher or lower rate: and we have given reasons for preferring the higher[3]. Perhaps we shall not be very wrong in assuming 25 miles as its average thickness between the upper and lower mean levels.

In equation (1) p is the whole force which resists motion upwards, and at the neutral zone it is equal to

$$g\rho h + \int_0^h f\,dx\,;$$

which consists of two parts, namely the weight of the column of rock above that zone and the frictional force above it.

Similarly p' in equation (2), taken between the neutral zone and the bottom of the crust, gives the whole force which resists motion downwards, and this is found to be

$$g\rho h + \int^k f\,dx.$$

The first term of this expression is the same as in the previous one, and is the weight of the column above the neutral zone; and the second term is the frictional force below the neutral zone. By the property of the neutral zone these second terms

[1] p. 102.

[2] "On the occlusion of gaseous matter &c.," "Journal of Iron and Steel Institute," no. II., 1881, p. 3.

[3] p. 16.

are equal, and will evidently increase with the thickness of the crust. So also will the first term. And, since the depth h of the neutral zone has been shown to be proportional to the thickness of the crust, it follows that the first term is proportional to that thickness, and the second possibly may be so. This however depends upon the constitution of the crust. It may therefore happen that $g\rho h + \int_0^h f\,dx$ may be greater for a small value of h when f is great, than for a considerably larger value of h when f is small, that is the strength of the crust may present a greater obstacle to shearing than its weight. It appears therefore that there must exist some relation between the amount of vertical shear and the strength of the crust.

By the favour of Messrs Macmillan and of the proprietors of "La Nature," one of the woodcuts is exhibited overleaf representing the results of some interesting experiments made by Prof. A. Favre of Geneva. The following description is from the translation given of Prof. Favre's article in "Nature," vol. XIX., p. 103. "I placed the layer of clay employed in these experiments on a sheet of caoutchouc, tightly stretched, to which I made it adhere as much as possible; then I allowed the caoutchouc to resume its original dimensions. By its contraction the caoutchouc would act equally on all points of the lower part of the clay, and more or less on all the mass in the direction of the lateral thrust......."

"The arrangement of the apparatus is very simple. A sheet of India rubber 16 mm. in thickness, 12 cm. broad, and 40 cm. long, was stretched, in most of the experiments, to a length of 60 cm. This was covered with a layer of potter's clay in a pasty condition, the thickness of which varied according to the experiments from 25 to 60 mm. It will be seen from the dimensions indicated that pressure would diminish the length of the band of clay by one-third. This pressure has been exerted on certain mountains of Savoy......."

"The strata, which appear to divide the masses of clay, and which are represented in the figures, are not really strata, but simply horizontal lines at the surface of the clay"—probably

Figure copied from "Nature," vol. XIX., p. 104, illustrating Professor A. Favre's experiment upon the compression of a layer of clay.

ruled along the side of the mass before the compression was allowed to act. After describing the varied appearances, he observes, "The strata are less strongly contorted in the lower parts than in the neighbourhood of the upper surface. They are disjoined in certain parts by fissures or caverns. They are traversed by clefts or faults inclined or vertical. All these deformations are the more varied, in that they are not similar on the opposite sides of the same band of clay."

It appears then that instead of producing contortion at the layer which adhered to the caoutchouc band, the compression was there relieved by mere thickening, and that contortion became more and more pronounced at greater distances from this layer. To this uncontorted layer our neutral zone would correspond. As the upper surface of the crust was approached, the contortion would become more marked, simple compression accompanied by upswelling characterising the disturbance of the strata nearer to the neutral zone. On the character of the disturbance below this we cannot safely speculate. But probably, on account of the continually increasing pressure, and the softening of the material by heat, the contortions would, as already explained[1], be of a more confused character than above it.

It must not however be concluded that, because Prof. Favre's experiments so happily illustrate the point in hand, it follows that the theory which he proposed to illustrate by them is the same as the one which is now advanced; for the object he had in view was to test the effect of secular cooling alone, in producing inequalities upon the earth's surface, without the hypothesis of a liquid substratum. We can however easily see, that such a theory would require the entire globe to be covered, like the caoutchouc band, by these corrugations; instead of their being arranged, as they are, in mountain chains, to say nothing of the inadequacy of that theory to account for any appreciable amount of compression, as has been demonstrated in Chapter VIII.

Prof. Favre's experiments may be fairly adduced to show how hopeless it would be to predicate the exact form which

[1] p. 176.

contortions would assume. Scarcely any conditions can be conceived more favourable to uniformity of result than those which he devised; and yet it is evident at a glance how varied was the result obtained at various points of the longitudinal section. In no respect do the experiments illustrate the case of nature more strikingly than in this apparent complexity of the disturbances. How much greater this would have been when we consider the unhomogeneity of the actual strata may be easily imagined. Yet it is certain that even in the experiment the contortion must have been governed by unerring laws resulting from a slight want of homogeneity in the material: and perhaps even, as the figures seem to suggest, from the local weakness caused by the horizontal lines, which had been traced upon the face of the clay before compression.

In the case of Prof. Favre's experiments the compression was uniform along the tract. In that which we are considering it would be otherwise. The pressure would increase until the crust gave way at one place. There it would be crushed upwards and downwards, above and below the neutral zone, until the thickening had attained a certain limit, which would depend upon the rigidity of the material, its weight, and probably also on the angle of repose. When this limit had been attained, a fresh region would be involved in the process; and this would adjoin the former. But as the lateral force expended itself, the thickness disturbed would become less and less, and the disturbance would gradually die out as it receded from that side from which the movement came.

Since the portion of the crust which lay within the limits of the original thickness would suffer greater compression than the raised and depressed portions above and below it, being more in the direct line of the pressure, we might expect that the flexures would hang over towards the side from which the movement came, thus causing inversions of bedding. This phenomenon is common on the flanks of mountain chains, and especially marked in the Appalachians. In the explanation usually given, the pressure has been supposed to have come from the side, away from which the flexures incline: so that if the flexures incline towards the west, the movement is supposed

to have come from the east[1]. According to the explanation now proposed the case would be the exact opposite, and if the flexures hang over towards the west, the movement came from the west. Where we find inverted bedding on both sides of an anticlinal, or a fan-shaped structure, we infer that a relative movement towards that region has occurred from the opposite sides, and this was illustrated in Mr Cadell's experiments[2].

We will now attempt to trace the formation of an elevated region by the compression of a tract of crust. We suppose that the compression has thickened the tract by crushing and corrugating the rocks of which it is composed, and that this thickening is greater about that part of the tract which was first compressed, and gradually subsides as it recedes from the region or regions from which the movement came. We also conclude that there will be what we have called a neutral zone, or level, within the crust, above which the material will on the average be sheared upwards, and below it downwards. It will correspond with the caoutchouc band in Professor Favre's experiments. The position of the neutral zone will depend upon the law, according to which the plasticity of the material changes at different depths.

We have however seen that its position below the surface lies probably at between one-third and one-half of the total thickness of the crust, and that this result, combined with our knowledge of the temperature at which granite has been formed, shows that the total average thickness probably exceeds twenty-one miles, and that this conclusion agrees well with the fact that the melting temperature of rock even in the dry way may be expected to occur at a depth of less than about thirty miles.

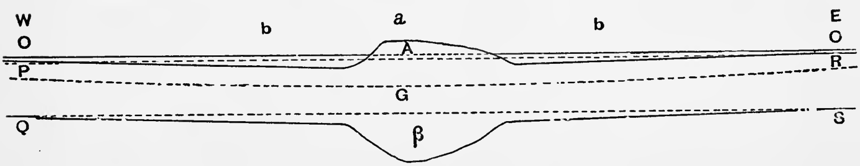
We will take therefore as the bases of our working hypothesis, that the undisturbed crust is about twenty-five miles thick, and that the neutral zone is at the depth of about ten

[1] Prof. Green's "Geology for Students," p. 377, with a reference to "Silliman's Journal," 1st Series, XLIX., 284.

[2] Paper read before the Roy. Soc. Edin., Feb. 20, 1888. Abstracted in "Nature," vol. XXXVII., p. 488, fig. 4, 1888.

miles from the upper and fifteen from the lower mean level. Consequently, where the tract is compressed, it is easily seen that the thicknesses of the elevated and depressed portions above and below the mean levels will be in the same ratio, viz. of 2 : 3. Since the compression along the tract acts in a line which falls within the crust it will not have any tendency to open fissures, but on the contrary to prevent their being formed or to close them if they exist, so that the subjacent fluid will not as a consequence of the compression gain access to, and overflow, the depressed parts $\Sigma (b)$ of the upper surface.

In the diagram suppose compression to have taken place by a movement of *PQ* towards *RS*. A portion of the crust will



Scale $\frac{1}{10}$ of an inch to 5 miles.
The upper line *OO* is the sea level.
The upper broken line is the upper mean level.
The middle broken line is the neutral zone.
The lower broken line is the lower mean level.
A the volume above the sea level.
β the volume below the lower mean level.
G is the centre of gravity of the tract.
The thickness of the crust is about 25 miles.
The length *PR* is 400 miles.
The height of *A* above the sea level is 5 miles.
The length of the compressed portion is here represented as about 100 miles.

have become thickened, accompanied with internal corrugation and disturbance: and the thickening will die out gradually towards *RS*. This corrugated portion is represented in the diagram as about 100 miles across, but it would have been better had space permitted to have drawn it wider.

If the neutral zone between *PQ* and *RS* were now to maintain its horizontality, we should have the volume elevated above the mean line *PR* : that depressed below the mean line *QS* :: 2 : 3. Such a condition of horizontality would require

that $\Sigma(b) = 0$ and $\Sigma(\alpha) = 0$. It would also give for the condition of floating equilibrium,

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta) - \Sigma(\alpha)} = \frac{2}{3},$$

which would require that $\frac{\sigma - \rho}{\rho} = \frac{2}{3}$, or 0·666, and the subjacent fluid would need to be $\frac{5}{3}$ times as dense as the crust. This is altogether unlikely. We have on the other hand hitherto assumed, on grounds likely to make σ too large, that $\frac{\sigma - \rho}{\rho} = 0{\cdot}104$. The crust must then be so rigid that it cannot bend or break (and the points Q and S are not fixed points, but may retreat from one another so as to give any requisite smallness to the curvature of the bent tract between them) or else it must either bend or break, and hollows corresponding to $\Sigma(b)$ be formed in which the ocean would exceed the average depth. It is not likely, from the nature of the case, that any hollows will be formed below, belonging to the series $\Sigma(\alpha)$. It will consequently take such a position as indicated in the diagram, in which the condition is intended to be approximately satisfied that

$$\frac{\Sigma(a) - \Sigma(b)}{\Sigma(\beta)} = 0{\cdot}104.$$

The volume A above the ocean level is supposed in the diagram to be about five miles high.

Here then we have exhibited the primary idea of the formation of a mountain ridge according to our theory, leaving the cause of compression for future discussion.

Let us next proceed to enquire what effects would be produced upon our tract by denudation, and the transference of sediment.

It has been shown[1] that

$$\Sigma(a) - \Sigma(b) = \frac{\sigma - \rho}{\sigma} klc + \frac{\mu}{\sigma}\{\delta l - \Sigma(d)\}.$$

Hence $\Sigma(a)$ is always greater than $\Sigma(b)$; so that even if sufficient material was denuded off $\Sigma(a)$ and carried into $\Sigma(b)$ to completely fill it up, $\Sigma(a)$ would not have been all carried

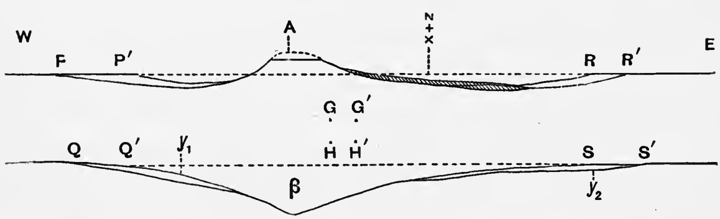
[1] Equation (1), p. 167.

away. This shows that, when once an elevated tract has been formed, it must be permanent. The presence of an ocean also, since the term in μ is necessarily positive, will render the excess of $\Sigma(a)$ over $\Sigma(b)$ greater than if there were no ocean.

It does not however follow that, after $\Sigma(b)$ is filled up, $\Sigma(a)$ may not be denuded down below the ocean level, which it must be carefully remembered is distinct from, and above, the upper mean level. All that we assert is that an elevation once formed must be permanent above the mean level.

Supposing the thrust to have come from beyond PQ, in the position which the tract has now assumed, there will be at first a certain amount of residual difference of horizontal pressure at PQ and RS, arising from the friction of the substratum, which has determined the distance to which the disturbance has reached. Moreover, at the time when the movement ceases, the horizontal thrust will still remain greater than if it had had only the weight of the crust to work against. Since however, with sufficient time allowed, any substance not absolutely rigid will yield to distortion, we may assume that eventually the horizontal pressures at PQ and RS will be reduced to what the mere weight would cause[1], and be equalized; and then we may reason about the tract, as if it were in equilibrium as a floating body.

As we have drawn our diagram the centre of gravity of the mass bounded by PQ and RS will be at G. And since the



In this diagram the vertical scale is double the horizontal. The latter is somewhat reduced from the scale of the previous diagram.

The shaded part is the sediment.

The ocean level is omitted; and no attempt has been made to represent the changes of level, which, although of the greatest importance, would have been scarcely visible upon the figure.

[1] See p. 169.

resultant of the vertical forces acts through G, it follows that if the cross stresses at PQ and RS are just such as the crust will bear, and equal, G will be equidistant from PQ and RS.

We have now arrived at the conception of a ridge, of which our diagram represents a section, steeper upon one side, say the west, than upon the other, the east. It is bordered by an ocean upon either side. The ocean extends beyond the limits of the tract disturbed, which is defined by P and R, and is there of its average depth. But there are channels, corresponding to $\Sigma(b)$, running parallel to the ridge, where the ocean is abnormally deep; and of these the deeper is upon the side where the ridge is steeper. That this will be the case is obvious, on account of the position of the centre of gravity of the tract being half way between PQ and RS.

When atmospheric influences begin to act on the ridge, and denudation comes into play, we may expect that the principal and longer streams will be formed on the east side, which is less steep. The crest of the ridge will then be lowered, the basset edges of the contorted strata exposed, and the material removed will be deposited on the low grounds, and along the shore line, more on the east side than on the west.

We must remember however that our diagram represents only a section across an isolated ridge. But it will be proper to contemplate the existence of other elevated tracts, which, either by rivers flowing from them, or by ocean currents, or by shore drifts, or by iceberg deposits, may contribute to the deposition of sediment to the east of the ridge. For instance, in the case of a section taken from Valparaiso to Buenos Ayres, we should need to consider the sediment brought down by the Rio de la Plata, which would not be derived from any area crossed by the line of section.

In endeavouring to trace the mechanical results of such denudation and deposition, let us call x the material denuded off the ridge A, and z that brought from some distant region and deposited along with x. If we assume the centre of gravity of the tract to be originally at G, then the denudation of the material x off A, and its transference towards the east, will shift this centre towards the east. The deposition of the additional

sediment z, brought from a distance and deposited along with or near x, will have a like additional effect. Let G' be the position to which this centre is shifted.

If the crust were flexible, this would produce deformation, and an alteration of curvature on either side of G'; but it would not tend to cause rotation. Since however it is only partially flexible, the deformation will proceed as far as the forces acting will carry it, and when they can bend it no further, it will afterwards behave as if it were rigid. Suppose at this stage that the centre of gravity of the displaced fluid is brought to H. This point will evidently be to the west of G'. The upward pressure of the fluid at H will tend to produce rotation round G', raising the tract to the west, and depressing it towards the east. One of two effects may be expected to result. Either the crust will break off at RS and perhaps also at PQ, where before the change of pressure it was only just able to bear the downward stress, or else, as in the diagram, the limits of the depressed tract will be shifted eastward to $P'Q'$ and $R'S'$. In either case, an additional volume of the crust will be depressed into the fluid towards the east, and the effect will be, that the centre of gravity of the displaced fluid will be also shifted towards the east; in which direction it will move, until equilibrium is restored by its arriving beneath G'.

The centre of gravity of the tract will thus continually travel eastwards, as more and more sediment is deposited in that direction. And since the centre of gravity is the point, about which the tract will tend to rotate under the action of the shifted and of the additional weight above, and of the fluid pressure beneath, it follows that, if the ends of the tract at PQ and RS are not tilted upwards and downwards, as would happen if the tract were rigid, there will be a kind of wave of elevation travelling from west to east, whose crest will carry beneath it the centre of gravity of the tract. We see then *a priori* that the region which receives the sediment from A will sink, but at the same time A from which it came will rise, and if there be additional sediment brought from distant regions, these effects, not only the sinking of the sedimented parts, but the rise of A, will be intensified. We might even expect, that

some of the sediment earliest deposited nearest the west, might be raised above the ocean by this rotational action alone.

We will now endeavour to obtain a somewhat closer insight into the character of the movements, in doing which we shall for simplicity neglect the weight of the ocean.

If a mass floats in a fluid, as we have supposed our tract virtually to do, the densities being respectively ρ and σ, we know that, whether the mass be rigid or not,

$$\rho \times \textit{whole volume} = \sigma \times \textit{volume immersed}.$$

And, since the centres of gravity of these volumes must be in the same vertical, if we multiply each side of the above by the distance of its corresponding centre from any assumed vertical we have, considering the moments about that vertical,

$$\rho \times \textit{moment of whole volume} = \sigma \times \textit{moment of volume immersed}.$$

Let A be the volume which before denudation stood above the ocean level, and, as already supposed, let a mass x be denuded off it, and carried down towards the east, and let another mass z be also brought from a distance and deposited along with x. This, as has been explained, will tend to depress the tract on the eastern side, and to raise it on the western, and our object is to determine as far as possible the situations and amounts of the depression and elevation.

Let y_2 be the additional volume of crust depressed beneath the lower mean level on the eastern side, and y_1 the volume by which the portion previously depressed is now diminished on the western side. The whole volume beneath the lower mean level will now be $\beta - y_1 + y_2$.

Let us call the horizontal distances, from the assumed vertical, of the centres of gravity of the volumes A, $x + z$, y_1, y_2, respectively $\overline{A}$, $\overline{x}$, $\overline{y}_1$, $\overline{y}_2$.

Let B be the volume of the tract which is exclusive of A, and C the volume at first below the effective level; $\overline{B}$ and $\overline{C}$ the distances of their centres of gravity from the assumed vertical line. Then the relation just proved gives us, before the transference of sediment,

$$\rho (A \,.\, \overline{A} + B \,.\, \overline{B}) = \sigma C \,.\, \overline{C}.$$

After the transference of x and deposition of z, A becomes

$A - x$, and we may suppose the distance of the centre of gravity of the diminished A to be unaltered by this change. B and $\overline{B}$ remain as before. C is diminished by y_1 and increased by y_2. Hence the relation of the moments will now be

$$\rho\{(A-x)\overline{A}+(x+z)\overline{x}+B.\overline{B}\}=\sigma(C.\overline{C}-y_1.\overline{y}_1+y_2.\overline{y}_2);$$

whence, subtracting,

$$\rho\{(x+z)\overline{x}-x\overline{A}\}=\sigma(y_2\overline{y}_2-y_1\overline{y}_1).$$

If then we assume the vertical to pass through the centre of gravity of the volumes y_1 and y_2, the second side will vanish, and we have

$$(x+z)\overline{x}=x\overline{A}.$$

This shows that the centre of gravity of the mass x before its removal, and of $x+z$ after their deposition in their new position, is in the same vertical with the centre of gravity of y_1 and y_2. Hence we can find the position of the centre of gravity of these latter volumes but not the position of the volumes themselves. It shows that the centre of gravity of y_1 and y_2 travels eastward at exactly the same rate as that of x before denudation and $x+z$ after deposition.

We may next compare the rate at which the centre of gravity of the whole tract travels, relatively to the vertical through the centre of gravity of y_1, y_2. For if M be the mass of the whole tract before the transference and deposition of sediment, and consequently $M+z$ afterwards, $\overline{M}$, $\overline{M}'$ the distances of their centres of gravity from an assumed vertical, then we have at first

$$M.\overline{M}=A.\overline{A}+B.\overline{B};$$

and afterwards

$$(M+z)\overline{M}'=(A-x)\overline{A}+(x+z)\overline{x}+B.\overline{B}.$$

But if we measure from the vertical which passes through the centre of gravity of y_1, y_2, we know that

$$x\overline{A}=(x+z)\overline{x};$$

$$\therefore\ (M+z)\overline{M}'-M\overline{M}=0.$$

If $z=0$, then $\overline{M}'=\overline{M}$.

So that by the transference of x alone, the centre of gravity of the whole mass remains at a constant distance from the vertical through that of y_1, y_2, not depending upon the amount of x.

If z is considerable compared to $\overline{M}$, we see that $\overline{M}'$ is less than $\overline{M}$, and the centre of gravity after transference is nearer to the vertical through that of y_1, y_2 than it was before. But since M must be always much larger than z, it will not approach very near to it. For instance, even if $z=\frac{1}{2}M$; $\overline{M}'$ would be $\frac{2}{3}\overline{M}$.

This result shows that the rotation round G', which will depress the centre of gravity of $x+z$, would not be likely to bring up any of this sediment above the sea level, unless $x+z$ was spread out very thin, so that its western boundary considerably overlapped the point G'.

But although an elevatory effect upon the newly deposited sediment can hardly be expected to result from the *rotation* of the tract round G', nevertheless there is a further consideration which leads us to expect that this consequence will follow upon the transference of sediment from a distance, for the following reason.

It is evident that we must regard the tract under consideration, whose volume was $kl(1+c)$ before it received the new accession z, as now become $kl(1+c)+z$. Hence, if $\Sigma(a)$ and $\Sigma(b)$ become by this accession $\Sigma(a_1)$ and $\Sigma(b_1)$ we have, neglecting the effect of the ocean[1],

$$\Sigma(a_1)-\Sigma(b_1)=\frac{\sigma-\rho}{\sigma}(klc+z);$$

$$\therefore \Sigma(a_1)-\Sigma(a)-\{\Sigma(b_1)-\Sigma(b)\}=\frac{\sigma-\rho}{\sigma}z.$$

We see then that the absolute volume above the upper mean level, and therefore probably also above the ocean, is in consequence of the accession of z increased by $\frac{\sigma-\rho}{\sigma}z$, or by about $\frac{1}{10}$th of the new accession of material to the tract.

In a similar way it appears that the volume below the lower mean, or y_2-y_1, must be increased by $\frac{\rho}{\sigma}z$, or by nine-

[1] See equation (1) on p. 167.

tenths of the new accession. Although therefore there will be an addition to the absolute volume above the upper mean level, and possibly above the ocean, there will be at the same time an addition of about nine times as much to the volume below the lower mean level. Or nine-tenths of the sediment brought down will disappear from view. This explains how it is, that much of the enormous quantity of sediment brought down by a great river appears to be as it were swallowed up in a bottomless abyss. It will be recollected, that this was one of the facts primarily adduced to show *a priori* the improbability of an entirely solid earth[1].

We appear to have now learnt all we are able about the character of the movements produced by the transference of the sediment. We cannot find the actual positions of the volumes y_1 and y_2, because these will depend upon the degree of rigidity of the crust. And even if we knew what this is, the determination would probably be impracticable. However, we can see that the less readily the crust replies by bending or breaking to the altered strain upon it, the less the curvature will be altered, and therefore the further will be the centres of y_1 and y_2 from the axis of rotation of the tract.

A consequence of the rotation round G' will be to give the tract a certain degree of inclination, or "hang" towards the east, so that the resultant pressure of the fluid will have a certain amount of eastward direction; and in assuming the new position of equilibrium there will be a general shifting of the whole tract eastwards. The result will be to subject the crust in the region of y_1 to tension, which may possibly open fissures downwards on the western side of the ridge.

The smaller the distance apart of y_1, y_2, the greater this "hang" will be for given values of them.

It is evident that the existence of y_1, the uptilted volume, depends entirely upon the crust possessing a certain degree of rigidity. If there were no rigidity there would be no tilting. A certain additional rigidity will be imparted to the tract as a whole, by the existence of a considerable ridge, which, with

[1] p. 130.

its accompanying depression, will act as a brace to prevent bending, strengthening the tract in the neighbourhood of the *quasi* fulcrum at the centre of gravity.

The removal of material off A by denudation, independently of its subsequent deposition towards the east, will have the effect of shifting G to the east, and causing the ridge to be lifted by the pressure of the fluid beneath. The mere degradation of a mountain chain, without taking into account the tilting caused by the weight of sediment in the bordering hollows, would consequently be accompanied by a corresponding rising of that part of the mass.

We have considered the effects of denudation and of the deposition of sediment. There remains the case of a tract being overflowed by basalt, which may be regarded as of the same density (σ) as the substratum. In this case the height of the tract would not be altered. For before the overflow, let m be the height of the surface above the effective level, and n the depth below it.

Then $$\rho (m + n) = \sigma n.$$

Now let a layer of basalt of density σ and thickness x overspread the surface, and let the immersed depth be thereby increased to $n + z$.

Then $$\rho (m + n) + \sigma x = \sigma (n + z).$$

Whence we see that $$x = z,$$

or the depth of the immersed part is increased by the thickness laid on the top. Consequently the height of the tract would be hydrostatically unaltered. There appears however to be some geological agency operating, which causes such areas to be elevated at the time of the extravasation of the basalt, and proves that the reservoir, from which the matter is derived, can be no mere local receptacle, or lake, of molten rock, out of which material is simply transferred from beneath to the surface without the volume being altered[1].

[1] See Prestwich on the "Agency of water in volcanic eruptions," "Proc. Roy. Soc.," no. 246, 1886, p. 168 of reprint.

CHAPTER XIV.

THE REVELATIONS OF THE PLUMB-LINE.

Mountains the "backbones" of continents—They have "roots"—These are revealed by the plumb-line—Pratt's calculation of the attraction of the Himalayas—The actual attraction less than that calculated—His attempted explanation of the discrepancy—Sir G. B. Airy's explanation more satisfactory—Pratt's reply shown to be inconclusive—The Pyrenees another instance—The results confirm the reasoning of the preceding chapter—The plumb-line reveals a greater density beneath oceans—Sir G. B. Airy's explanation meets Prof. Darwin's argument for solidity founded on the "stresses caused by continents and mountains"—Roots of mountains need not be melted off.

We have now arrived at a point at which we may begin to test our theories by comparing the results of them with observed phenomena.

In noticing the section on page 184, which is roughly drawn to scale, we are at once struck with the importance assumed by a mountain range with reference to a continent, when the depression below the lower mean level is taken account of, as well as the elevation which appears above the level of the sea. The latter has been usually regarded as the sufficient expression of the magnitude of the range, and the importance of the highest mountains has accordingly been minimized, and they have been called mere wrinkles upon the surface of the globe. Wrinkles no doubt they are; but they must be regarded as thicker, than they appear above ground to be, in the ratio of perhaps 5 to 2, and they may be fairly called the backbones of continents.

According to the consequences which we have traced, as resulting from their denudation, we are also led to look upon a mountain range as truly the parent of a continent, and to consider the latter as gradually evolved out of the former: so that, even if there is no present range of sufficient altitude to be dignified as a mountain chain, such must have at some former time existed.

We also see why a range of mountains as a rule skirts an ocean shore, or at least did so when it was first formed. And we also see why it usually presents its steeper face towards the ocean.

It is not a little remarkable that popular expressions, even when grounded on no scientific basis, have often a foundation in truth which justifies them. Such an expression is that which speaks of the "roots of the mountain." This is amply justified by our theory; for every mountain chain must possess a corresponding protuberance projecting downwards into the fluid substratum, which may be aptly so named.

These roots of the mountains, though they cannot be seen, can in a most remarkable manner be felt by the plumb-line; and this certainly affords a strong support to the truth of our theory. "The attraction of the Himalaya Mountains, and of the elevated regions lying beyond them, has a sensible influence upon the plumb-line in North India. This circumstance has been brought to light during the progress of the great trigonometrical survey of that country. It has been found by triangulation that the difference of latitude between the two extreme stations of the northern division of the arc," that is, between Kalianpur and Kaliana, "is 5° 23′ 42″·294, whereas astronomical observations show a difference of 5° 23′ 37″·058, which is 5″·236[1] less than the former[2]." The latitude of Kalianpur is 24° 7′ 11″ and that of Kaliana 29° 30′ 48″[3].

[1] "This is the difference as stated by Colonel Everest in his work on the measurement of the meridianal arc of India published in 1847. See p. clxxviii."

[2] "On the Attraction of the Himalaya Mountains, and of the elevated regions beyond them, upon the Plumb-line in India." By the Venerable John Henry Pratt, M.A., Archdeacon of Calcutta. "Phil. Trans. Roy. Soc.," vol. 145, p. 53.

[3] *Ibid.* p. 56.

It having thus appeared that the plumb-line was attracted at the station near the mountains in a direction towards the mountains, thereupon Archdeacon Pratt set himself what appeared the herculean task of actually calculating what the effect of the attraction of that great mountainous region ought to be, and in a second paper upon the same subject he says, "My calculation has been before the public three years; and, though some small numerical errors have been detected, they are not of sufficient importance to affect the result; and the data I have every reason for believing to be correctly taken, as the Surveyor-General—who first called my attention to the subject in 1852, as an unsolved difficulty in the operations of the great Trigonometrical Survey of India—has been requested to forward to me any corrections which may appear to him to be advisable, and none have been sent[1]." The result at which he arrived was, that the attraction of the mountainous region ought to have made, between the true and astronomically observed differences of latitude of the extreme stations, a discrepancy of $15''\cdot885$ instead of $5''\cdot236$ as it did. That is to say, the effect of the attraction of the mountains was in fact considerably less than it ought to have been, according to what their mass above the sea should produce, taking 2·75 from Maskelyne's determination for Schehallion for the density of the attracting mass. Thus the mountains' attraction not only accounted for the discrepancy discovered by the Survey, but accounted for too much. They ought to have attracted the plumb-line more than they did.

The Archdeacon then applied himself to explain this unexpected anomaly, and the conclusion he came to in his first paper was, that the assumed ellipticity $\frac{1}{300\cdot8}$ was too large for the Indian arc, the curvature there having been, as he thought, probably increased by the upheaving of the mountains. This

[1] Second Paper by Archd. Pratt. General Walker, the Superintendent of the Trigonometrical Survey of India, stated in his Presidential Address to the Geographical section of the Br. Assoc. 1885, that Pratt's calculations were "based on reliable data and were indubitably correct." "Nature," vol. XXXII., p. 486, 1885. "Phil. Trans. Roy. Soc.," vol. 149, p. 746.

would have the effect of making the difference calculated upon the triangulation larger, so as to compensate for the effect of the mountains[1].

However the very next paper in the "Phil. Transactions[2]" disposes of the difficulty in a very satisfactory manner. It was communicated by the late Astronomer Royal about a year after Archdeacon Pratt's great paper appeared. This paper is short and simple, and with the author's permission the following passages are extracted from it.

"Although the surface of the earth consists everywhere of a hard crust, with only enough of water lying upon it to give us everywhere a *couche de niveau*, and to enable us to estimate the

[1] If λ be the amplitude of the arc (*i.e.* the difference between the latitudes of its extreme stations), l its length, μ the latitude of its middle point, and ϵ the ellipticity, the usual formula is

$$\frac{\text{length of arc } (l)}{\text{equatorial radius } (a)} = \lambda - \tfrac{1}{2}\epsilon\,(\lambda + 3 \sin\lambda \cos 2\mu).$$

λ is also affected by the mountain attraction, which we may call H.

Consequently

$$\lambda = f\,(a, l, \mu, H, \epsilon).$$

Consequently if the observed value of λ be not such as accords with the usually assumed value of ϵ when H is determined, some different value of ϵ may be found which will bring λ into accordance with its observed value. And this is the mode in which Archd. Pratt proposed to explain the difficulty.

Eliminating a from the above equation by means of another measured arc, the latitude of whose middle point is M, he obtains the following approximate formula:

$$d\epsilon = \frac{d\lambda}{\lambda}\,\frac{2}{3\,(\cos 2\mu - \cos 2M)}.$$

In order to give this as small a value as practicable he takes the Russian arc for the other, for which $M = 70^0$, and after applying the formula to the case in hand, finds

$$d\epsilon = \frac{1}{39585} = \frac{\epsilon}{132},$$

if we put $\frac{1}{300}$ for ϵ.

"Hence for an error of $5''{\cdot}236$ in defect in the amplitude, the effect on the ellipticity will be to diminish it by $\frac{5{\cdot}236}{132}\,\epsilon = \frac{\epsilon}{25}$ nearly, or by nearly $\frac{1}{25}$ part of its whole value under the most favourable circumstances."

[2] "On the Computation of the Effect of the Attraction of Mountain-masses as disturbing the Apparent Astronomical Latitude of Stations in Geodetic Surveys." By G. B. Airy, Esq., Astronomer Royal. Feb. 15, 1855. "Phil. Trans. Roy. Soc.," vol. 145, p. 101. See also a lecture by Sir G. B. Airy, "On the Interior of the Earth." "Nature," vol. XVIII., p. 41, 1878.

heights of the mountains in some places, and the depths of the basins in others; yet the smallness of those elevations and depths, the correctness with which the hard part of the earth has assumed the spheroidal form, and the absence of any particular preponderance either of land or of water at the equator as compared with the poles, have induced most physicists to suppose, either that the interior of the earth is now fluid, or that it was fluid when the mountains took their present forms. This fluidity may be very imperfect, it may be mere viscidity; it may even be little more than that degree of yielding which (as is well known to miners) shows itself by changes in the floors of subterraneous chambers at a great depth where their width exceeds 20 or 30 feet, and this yielding may be sufficient for my present explanation."

He then proves that a table-land, say of 100 miles broad and two miles high, could not rest upon the surface of a crust ten miles thick.

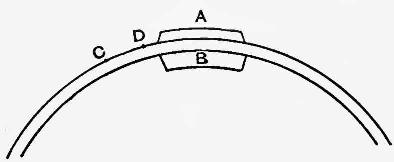
"Now I say that this state of things is impossible, the weight of the table-land would break the crust through its whole depth from the top of the table-land to the surface of the lava" (which is merely used as a generic term for a heavier subjacent non-solid material), "and either the whole or only the middle part would sink into the lava." To prevent this happening, with the assumed dimensions, it would be necessary that the cohesion of the rock should be sufficient to support a suspended column of the same twenty miles long. "I need not say that there is no such thing in nature. If instead of supposing the crust ten miles thick, we had supposed it 100 miles thick, the necessary value of the cohesion would have been reduced to $\frac{1}{5}$th of a mile nearly. This value would have been as fatal to the supposition as the other[1]."

"Yet the table-land does exist in its elevation, and therefore it *is* supported from below. What can the nature of its support be? I conceive there can be no other support than that arising from the downward projection of a portion of the earth's light crust into the dense lava; the horizontal extent of that projection corresponding rudely with the horizontal extent of

[1] "Phil. Trans. Roy. Soc.," vol. 145, p. 102.

the table-land, and the depth thus gained is roughly equal to the increase of weight above from the prominence of the table-land[1]."

It will be at once seen that this reasoning produces an arrangement of the crust upon the subjacent fluid exactly analogous to that which has been arrived at in the preceding chapter on the hypothesis of lateral compression. Of course we from our point of view cannot contemplate the independent formation of a table-land apart from the downward projection, so that it should afterwards break and form one. But it does not seem that it concerned Sir G. B. Airy to show that the one cannot be formed without the other. The object he had in view was to explain how this arrangement of matter would produce a smaller deviation of the plumb-line. He says, "It will be remarked that the disturbance depends on two actions; the positive attraction produced by the elevated table-land, and the diminution of attraction produced by the substitution of a certain volume of light crust (in the lower projection) for heavy lava. The diminution of attractive matter below, produced by the substitution of light crust for heavy lava, will be sensibly equal to the increase of attractive matter above. The difference of the negative attraction of one and the positive attraction of the other, as estimated in the direction of a line perpendicular to that joining the centres of attraction of the two masses (or as estimated in a horizontal line), will be proportional to the difference of the inverse cubes of the distances of the attracted point from the two masses."



"Suppose then that the point C is at a great distance, where nevertheless the positive attraction of the mass A, considered alone, would have produced a very sensible effect on the astro-

[1] *Ibid.* p. 103.

nomical latitude, as ten seconds. The effect of the negative attraction of B will be $10'' \times \frac{CA^3}{CB^3}$; and the whole effect will be $10'' \times \frac{CB^3 - CA^3}{CB^3}$, which probably will be quite insensible.

"But suppose that the point D is at a much smaller distance, where the positive attraction of the mass A would have produced the effect n''. The whole effect, by the same formula, will be $n'' \times \frac{DB^3 - DA^3}{DB^3}$, or $n'' \times \left(1 - \frac{DA^3}{DB^3}\right)$; and as in this case the fraction $\frac{DA}{DB}$ is not very nearly equal to 1, there may be a considerable residual disturbing attraction. But even here, and however near to the mountains the station D may be, the real disturbing attraction will be less than that found by computing the attraction of the table-land alone."

The extreme felicity of the above explanation of a remarkable phenomenon and the lucidity with which it is stated must be sufficient apology for the length of the extract. One remark more must be added because it is very pertinent to the subject and reasoning of the preceding chapter. "It is supposed that the crust is floating in a state of equilibrium. But in our entire ignorance of the *modus operandi* of the forces which have raised submarine strata to the tops of high mountains, we cannot insist on this as absolutely true. We know (from the reasoning above) that it will be so to the limit of *breakage* of the table-lands, but within those limits there may be some range of conditions either way[1]." We believe however that since this passage was written geology has made some advance towards explaining the *modus operandi* of the elevatory forces; but the proviso respecting the limits of breakage of the rocks is of great importance and must not be forgotten.

Archdeacon Pratt replied to the Astronomer Royal's paper three years later[2]. He brought against the explanation proposed three objections, not one of which appears in the present state of

[1] "Phil. Trans. Roy. Soc.," vol. 145, p. 104.

[2] "On the Deflection of the Plumb-line in India, &c." "Phil. Trans. Roy. Soc.," vol. 149, p. 745.

our knowledge to be of weight[1]. (1) It supposes the thickness of the earth's solid crust to be considerably smaller than Mr Hopkins concluded it to be. This objection has been disposed of[2]. (2) It assumes that the crust is lighter than the fluid on which it rests, whereas in becoming solid we should expect it to contract and become more dense. This argument is answered by Mr Whitley's experiments[3], and is contrary to Waltershausen's most reasonable theory[4] that the successive couches increase in density according to their chemical composition. (3) If every protuberance outside a thin crust must be accompanied by a protuberance inside down into the fluid mass, then wherever there is a hollow as in deep seas in the outward surface, there must be one also in the inner surface of the crust corresponding to it; thus leading to a law of varying thickness which no process of cooling could have produced. To this it may be replied that, the suboceanic crust is undoubtedly more dense than the continental, but we are not obliged to suppose that differences of density between the two are due solely to processes of cooling. We shall discuss the relations between the suboceanic and continental crust as regards density and thickness in Chapter XVII. In spite however of these three objections, which the Archdeacon believed to be fatal to the form in which the Astronomer Royal offered his explanation of a deficiency of matter below the mountains, the Archdeacon adopts in the remainder of this paper another form of the same theory, apparently preferring it altogether to his former hypothesis of an increased ellipticity for the Indian arc.

It is not a little remarkable that a conclusion similar to Airy's regarding the Himalayas had previously been arrived at by Petit for the Pyrēnees. For Humboldt refers to a memoire by him; "Sur la latitude de l'Observatoire de Toulouse, la densité moyenne de la Chaine des Pyrénées, et la probabilité qu'il existe un vide sous cette chaine[5]."

We have thus established our assertion that the roots of the mountains can be felt by means of the plumb-line.

[1] *Ibid.* p. 747. [2] p. 38. [3] p. 45. [4] p. 33.

[5] "Comptes rendus de l'Acad. des Sc.," t. xxix., 1849, p. 730. See "Cosmos," Sabine's translation, vol. 4, note 29 to p. 33, 1858.

The converse proposition ought to be true, that where the land is not elevated above the sea, there the heavier substratum ought to be within a less distance of the surface, and the consequence ought to be that attraction at the surface should be greater. This also is known experimentally to be the case. The subject is discussed by Archdeacon Pratt in the fourth edition of his "Figure of the Earth." He establishes the fact, but raises upon it theoretical conclusions not warranted by geology. There can however be no doubt that he has demonstrated the existence of increased density beneath the oceans. He says "in fact the density of the crust beneath the mountains must be less than that below the plains and still less than that below the ocean bed." "That part of the theory * * * * is well illustrated by the existence of a whole hemisphere of water, of which New Zealand is the pole, in stable equilibrium. Were the crust beneath only of the same density as that beneath the surrounding continents, the water would be drawn off by attraction and not allowed to stand in the undisturbed position it now occupies[1]." The greater density must then clearly exist; but it will appear in the seventeenth chapter, that the phenomena are not sufficiently explained by supposing the lighter crust thinner, and the heavier substratum nearer the surface in those regions.

Professor Darwin read a paper in June, 1881, before the Royal Society, "On the stresses caused in the interior of the earth by the weight of Continents and Mountains[2]." It proceeds upon the hypothesis that "the existence of dry land proves that the earth's surface is not a figure of equilibrium appropriate for the diurnal rotation. Hence the interior of the earth must be in a state of stress, and as the land does not sink in, nor the sea-bed rise up, the materials of which the earth is made must be strong enough to bear this stress." After describing the nature of the investigation, he concludes that it "must be regarded as confirmatory of Sir William Thomson's view, that

[1] "Figure of the Earth," 4th ed., Art. 192, pp. 201, 202.

[2] "Phil. Trans. Roy. Soc.," 1882, Part I., p. 187. Also "Proc. Roy. Soc.," vol. XXXII., p. 432, 1881.

the earth is solid nearly throughout its whole mass. According to this view, the lava which issues from volcanoes arises from the melting of solid rock, existing at a very high temperature at points where there is a diminution of pressure[1], or else from comparatively small vesicles of rock in a molten condition."

It will be seen that the facts, upon which Professor Darwin based his investigation, are the same as were perceived by Sir G. B. Airy to require explanation. Admitting that the materials of the earth cannot be strong enough to bear the stress, if his explanation be accepted, the stresses arising from the inequalities of the surface can exist only within the elevated tracts themselves, which possess no doubt sufficient rigidity to keep them from flowing down into flatness. If the crust is of appropriately varying thickness in different parts of the land surfaces, and denser beneath the oceans, and floats in equilibrium on a fluid substratum, it cannot transmit any unequal stresses to the subjacent parts.

It may possibly be objected to the theory of downward protuberances projecting into a molten interior, that they would be soon melted off. But it has been proved in the sixth chapter that the hypothesis of a thin crust necessitates that a layer of the substratum is, on the whole, in process of being gradually congealed on to, and so thickening, the crust; and that convection currents of some kind must exist in it. If that is the case, heat will be supplied to the bottom of the crust more rapidly where the ascending currents impinge, and less rapidly where the descending leave it. It is rather probable than not, that the roots of the mountains may correspond to the latter situations. But if there should be melting off of the roots, it would be a slow process; and we know that the subaerial protuberance, that is the mountain itself, although continuously being degraded by atmospheric agency, is not yet levelled down. There may therefore, equally well, not have been time for the root, ten times as large, to be melted off. Still further the root, consisting of acid rocks, would be less fusible than the more basic substratum.

[1] See the Author's Paper on "The Elevation of Mountains by Lateral Pressure." "Trans. Cambridge Phil. Soc.," vol. XI., Pt. III., p. 504. 1871.

CHAPTER XV.

THE REVELATIONS OF THE PENDULUM.

Pendulum operations in India—Corrections for height and latitude—Local attraction—Found to be generally less than the visible mountain masses would produce—General Walker's summary of results—The Author's paper in The Philosophical Magazine—Table of effects at stations on the Indian arc of the Meridian—Explanation of the table—Effect of elevated tracts in disturbing the sea level—Mean sea level defined—Disturbance small—Calculated and observed local attractions compared—Results confirm the theory of hydrostatic equilibrium—Geological structure of Peninsular India—Local attraction there—Conclusions from the same.

THE discovery of an unexpected deficiency in the horizontal attractive force of the region covered by the Himalayan range as related in the preceding chapter was followed by the undertaking in 1865 of an extensive series of pendulum observations by which the relative vertical force of gravity at a large number of stations in India and a few elsewhere were determined. A full description of these is to be found in volume V. of the "Account of the Operations of the Great Trigonometrical Survey of India[1]." What was done was in principle this:—At certain stations of the Survey, of which the height and position had been already determined, the mean number of swings, called the "vibration-number," was observed, which in twenty-four hours were made by two pendulums which at the equator would have made

[1] Prepared under the directions of Major-General J. T. Walker, C.B., R.E., F.R.S. Calcutta, 1879.

about 86,000 swings in the same interval[1]. (It will be remembered that the number of seconds in twenty-four hours is 86,400.) In this manner the force of gravity at each station could be compared.

The effect of height above the sea level is to diminish the attractive force of the sphere upon the pendulum, because it is by so much further from the earth's centre. It also subjects the pendulum to an increased centrifugal force as being further from the axis of the earth's rotation, but this effect is too small to be important. On both these accounts the acceleration of the pendulum is diminished, and it tends to vibrate more slowly. On the other hand the visible mass of an elevated region, being an excrescence on the mean sphere and also very near the pendulum, increases the acceleration and causes it to vibrate more quickly. The balance of these several effects may then be calculated for any station, and after being allowed for, the vibration-number so corrected would, if no disturbing cause of a hidden kind existed, have tallied exactly with that belonging to the latitude of the station. It turned out, however, that it was usually different, and for the most part in defect, often to a considerable extent. "There appears to be no escape from the conclusion that there is a more or less marked negative variation of gravity over the whole of the Indian continent, and that the magnitude of this variation is somehow connected with the height[2]."

"Pratt's calculations had reference only to the visible mountain and oceanic masses and their attractive influences—the former positive, the latter negative—in a horizontal direction; he had no data for investigating the density of the crust of the earth below either the mountains on the one hand, or the bed of the ocean on the other. The pendulum observations furnished the first direct measures of the vertical force of gravity in different localities which were obtained, and these measures revealed two broad facts regarding the disposition of the invisible matter below; first, that the force of gravity diminishes as the moun-

[1] "Account of the Operations of the Great Trigonometrical Survey of India," vol. v., p. [129].

[2] *Ibid.* p. [142].

tains are approached, and is very much less on the summit of the highly elevated Himalayan table-lands than can be accounted for otherwise than by a deficiency of matter below; secondly, that it increases as the ocean is approached, and is greater on islands than can be accounted for otherwise than by an excess of matter below. Assuming gravity to be normal [in amount] on coast lines, the mean observed increase at the island stations was such as to cause a seconds' pendulum to gain three seconds daily, and the mean observed decrease in the interior of the Continent would have caused the pendulum to lose $2\frac{1}{2}$ seconds daily at stations averaging 1,200 feet above the sea level, 5 seconds at 3,800 feet, and about 22 seconds at 15,400 feet—the highest elevation reached—in excess of the normal loss of rate due to height above the sea[1]."

The Author, hoping to obtain quantitative results, corroborative or otherwise, of the correctness of the theory of the hydrostatic equilibrium of disturbed regions of the crust as detailed in the XIIIth chapter, made a number of calculations on this subject, which were published in 1886[2]. The first step was to determine what would have been the difference between the vibration-number at a given station, and the vibration-number at Punnæ (the station at the southern extremity of the Peninsula), taking account solely of the height of the station, and of the difference of latitude from that of Punnæ. This was next compared with the observed difference between the vibration-numbers at the two places, and the want of agreement between the two differences—the calculated and the observed—showed how many vibrations *per diem* needed to be accounted for by peculiarities of local attraction. Reference was then made to the number of vibrations *per diem*, which the estimates of the survey officers showed to be attributable to the attraction

[1] Address by General J. T. Walker, C.B., LL.D., F.R.S., F.R.G.S., to the Geographical Section, British Assoc., 1885. Reported in "Nature," vol. XXXII., p. 486.

[2] "On the Variations of Gravity at certain stations of the Indian Arc of the Meridian in Relation to their Bearing upon the Constitution of the Earth's Crust." By Rev. O. Fisher, M.A., F.G.S. "Philosophical Magazine" for July, 1886. Taylor and Francis, London.

of the elevated region where the observation was taken, that is, to the attraction of the visible masses. The discrepancy then afforded a measure of the effect, which was undoubtedly to be attributed to a variation in density in the hidden masses below.

Let us illustrate the above by taking as an instance Moré, the most northern and most lofty station where observations were made. On account of the height of Moré, namely 15,408 feet, and of the difference of latitude, namely 25° 6′ 11″, there ought to have been 0·59 fewer swings of the pendulum *per diem* at Moré than at the sea level at Punnæ. There were however in fact 1·67 more. Consequently the difference between the difference there was, and the difference there ought to have been, irrespective of local attraction, amounted to 2·26 swings. This then was the number which local attraction needed to account for. But the number of swings, which the estimates of the surveyor show that the visible masses of the surrounding mountain region would have produced, was 23·45. It follows that the mountainous mass produced 23·45 – 2·26, or 21·19, fewer swings *per diem* than might have been expected. The import of this result is exactly of the same kind as that described in the preceding chapter as revealed by the plumb-line, namely the existence of a deficiency of density in the hidden parts of the earth's crust beneath the Himalayan range.

The table over leaf gives the results of the comparison, as just exemplified in the instance of Moré, for all the stations near the meridian arc.

The first six columns involve no hypothesis beyond that the curve, which represents the Indian meridian, does not sensibly depart from that which best represents the Earth as a whole, which Colonel Clarke considers may be assumed[1]; and that the sea level is not affected by local attraction. The fifth column is consequently the simple statement of the fact that local attraction at each station must be such as to account for so many swings of the pendulum *per diem* relative to the number at Punnæ reduced to sea level. The sixth column, taken from the account of the pendulum-operations[2], involves the further suppositions that the forms and positions of the attracting masses

[1] "Account, &c.," p. xxxii. [2] *Ibid.* pp. xxix. and [187].

VARIATIONS OF GRAVITY IN INDIA.

Stations near the Meridian of the Trigonometrical Survey.	1. North latitude.	2. Height in feet.	3. Observed difference of vibration-numbers from 85982·95, being that for sea-level at Punnæ.	4. Calculated difference for height above sea and latitude, not regarding local attraction.	5. Number of swings to be accounted for by excess of local attraction over that at Punnæ at sea-level, *i.e.* (3) – (4).	6. Number of swings which the elevated masses ought to have produced.	7. Deficiency due to diminished density beneath the mountains (5) – (6).
	° ′ ″						
Punnæ	8 9 28	48				0·07	
Kudankolam	8 10 21	168	– 0·39	– 0·47	+0·08	0·26	– 0·18
Mallapatti	9 28 45	288	– 0·35	+ 0·37	–0·72	0·44	– 1·16
Pachapaliam	10 59 40	971	– 0·67	– 0·36	–0·31	0·50	– 0·81
Bangalore, S.	13 0 41	3118	– 4·46	– 5·61	+1·15	4·83	– 3·68
Bangalore, N.	13 4 56	3009	– 3·57	– 5·43	+1·86	4·66	– 2·80
Namthábád	15 5 52	1173	+ 4·76	+ 5·80	–1·04	1·82	– 2·86
Kodangal	17 7 57	1914	+ 8·06	+ 7·06	+1·00	2·96	– 1·96
Damargida	18 3 17	1946	+ 8·09	+ 8·88	–0·79	3·01	– 3·80
Somtana	19 5 0	1714	+13·32	+12·39	+0·93	2·62	– 1·69
Badgaon	20 44 23	1120	+19·31	+18·99	+0·32	1·73	– 1·41
Ahmadpur	23 36 21	1693	+25·26	+24·47	+0·79	2·55	– 1·76
Kalianpur	24 7 11	1763	+27·41	+25·66	+1·75	2·73	– 0·98
Pahargarh	24 56 7	1641	+28·15	+28·57	–0·42	2·54	– 2·96
Usira	26 57 6	810	+38·36	+38·21	+0·15	1·14	– 0·99
Datairi	28 44 5	717	+43·78	+44·47	–0·69	1·11	– 1·80
Kaliana	29 30 55	810	+44·30	+46·58	–2·28	1·25	– 3·53
Nojli	29 53 28	879	+44·67	+47·56	–2·89	1·36	– 4·25
Dehra	30 19 29	2242	+39·91	+43·41	–3·50	3·18	– 6·68
Mussoorie	30 27 41	6920	+28·64	+24·58	+4·06	9·54	– 5·48
Moré	33 15 39	15408	+ 1·67	– 0·59	+2·26	23·45	–21·19

above the sea level have been correctly estimated relative to the station, and that their density is half the mean density of the earth, the sea level being supposed not to be disturbed by attraction. It will therefore appear that, although giving further information, it contains more elements of uncertainty than affect the numbers in the former columns. It gives the effect at the station, which the attraction of the masses elevated above the sea level would of themselves have produced.

When we compare the numbers in column 5 with these numbers, it is seen at a glance how much smaller local attraction actually is than it might be expected to be. It is no doubt true that, if allowance has to be made for a rise in the sea level, it would make the heights of the masses above the mean level greater, and consequently diminish the numbers in column 4, and increase those in 5; but at the same time it would increase the masses referred to in column 6; so that the excess of the numbers in that column over those in column 5 would not be thereby explained. But, what is still more remarkable, column 5 shows that local attraction is in some instances actually negative, where from the size and distribution of the masses it might be expected to be large and positive, as shown in column 6. This is notably the case, especially at Dehra, and to a less extent at Nojli, while at Moré, the most elevated station (15,408 feet), local attraction is reduced from the amount, which ought to be equivalent to cause 23·45 additional swings of the pendulum *per diem,* to what would produce 2·26 only.

The seventh column shows the negative effect of the deficiency in density in the crust of the earth beneath the mountains. Suppose for example that the mountains were carted away, and the surface of the mean sphere at the level of the ocean were laid bare. The average number of swings made by the two pendulums used would, directly under each station, have been less by the number given in column 7, than the average number made by them at sea level at Punnæ[1].

[1] The numbers given in Table II. at pages [146], [147] "Account &c.," have been subjected to a further reduction to compare them with the vibration number at the equator instead of Punnæ for a pendulum swinging seconds. When so reduced they are termed "the variation of gravity" at the station.

In discussing the effect upon the local variation of gravity arising from the supposed constitution of the crust, the first question which presents itself is, to what extent the sea-level will be affected. Pratt has an article (200) upon this; but the following proof is suggested.

Suppose generally that there is a mass, whose volume is M and density ρ, situated exterior to the Earth, and beneath it a mass, whose volume is R and density μ, within the Earth; and suppose that R is enveloped by the stratum whose density is σ.

Let $\frac{\rho M}{D}$ and $\frac{\mu R}{D'}$ be the potentials of the masses M and R at a point on the surface of the disturbed sea-level; $\frac{E}{r}$ the potential of the Earth at the same point. Then, supposing the space occupied by R to be vacant, the potential of the Earth will become $\frac{E}{r} - \frac{\sigma R}{D'}$. Hence, when we take into account all the masses which contribute to form the potential, recollecting that their sum at every point at the surface of the ocean must be constant, we have, upon restoring R,

$$\text{constant} = \frac{\rho M}{D} + \frac{\mu R}{D'} + \left(\frac{E}{r} - \frac{\sigma R}{D'}\right),$$

$$= \frac{\rho M}{D} - \frac{(\sigma - \mu) R}{D'} + \frac{E}{r}.$$

Let $r = c + \delta c$, where c is the mean radius, and δc the elevation of the water by the attraction of the masses M and R. Then, neglecting small terms, we have

$$\text{constant} = \frac{\rho M}{D} - \frac{(\sigma - \mu) R}{D'} + \frac{E}{c}\left(1 - \frac{\delta c}{c}\right).$$

The determination of the constant will depend upon what we assume to be the mean radius. If we were to assume it to be the radius to the nodal circle, where the surface of the disturbed water intersects the surface that the water would present if it was not disturbed, then the constant would have to be determined by the condition that the volume of the water remained unaltered. But this would not give what we want, which is the disturbance of the water above the existing mean

level of the ocean as determined by observation. In the case of the Himalayas, the surveyors avoided going sufficiently near the mountains for the plumb line to be sensibly affected. Hence it appears that we ought to consider the level to be unaffected at localities where the surveyors considered the ocean surface to be sensibly spherical, and to occupy its mean position; that is, where they regarded δc as non-existent, and consequently the effect of the masses as *nil.*

Determining the constant on this understanding we get,

$$\text{constant} = \frac{E}{c}.$$

$$\therefore \frac{E}{c^2}\delta c = \frac{\rho M}{D} - \frac{(\sigma - \mu) R}{D'}.$$

But
$$\frac{E}{c^2} = g:$$

$$\therefore \text{ elevation of water} = \frac{1}{g}\left\{\frac{\rho M}{D} - \frac{(\sigma - \mu) R}{D'}\right\}.$$

If the included mass had been more dense than the enveloping stratum, we should have had

$$\text{elevation of water} = \frac{1}{g}\left\{\frac{\rho M}{D} + \frac{(\mu - \sigma) R}{D'}\right\}.$$

We see, then, that the included mass R, if less dense, will have the effect of diminishing the elevation of the sea-level, and if more dense of increasing it. It may turn out, therefore, that our hypothesis of hydrostatic equilibrium will be found to accord with a very slight change in the sea-level[1].

Now a slight change in the sea level implies a slight change in the position of the plumb line; and it was this which formed

[1] The above value of the arbitrary constant agrees with that given by Pratt. Prof. Woodward, of the U.S.A. Geol. Survey, shows that, on the supposition of a mass being placed upon the surface of a centrobaric sphere covered with water, and the disturbance being referred to the original surface of the water, this value would not be correct: and he calculates a formula of great complexity, which is especially applicable to a cap of ice at a former glacial epoch. "On the form and position of the sea level as dependent on superficial masses." "Annals of Mathematics," vol. II., Nos. 5, 6, and vol. III., No. 1. Charlotteville, Va., Agents, Westermann, New York, 1886, 7. See also "Sixth Annual Report of the U.S.A. Geol. Survey," 1884, 5; p. 291.

the basis, on which Sir G. B. Airy grounded his theory of the hydrostatic mode of support of the elevated masses.

It will be sufficient in this place merely to recapitulate the results, which the author obtained in his article in the "Philosophical Magazine," because that publication is generally accessible, and the calculations are of a technical character, having in themselves no special interest.

And first, with regard to the change in the sea level which an elevated region would produce, if it were supported in hydrostatic equilibrium. Pratt says that, "for problems of this kind, the Himalayas may be considered as a vast table land about three miles high." Suppose that there was a canal cut from the coast line to beneath a station a good way from the edge of the table land. Then, if we take the thickness of the undisturbed crust to be 25 miles, and the densities of the crust and "root" to be 2·68, while that of the substratum is 2·96, it comes out that the rise of the water in such a canal would be only about 17 feet, if the table land was 3 miles high, which is the height of Moré. It would be less than 90 feet, if the plateau was 5 miles high[1]. These are very much smaller than the usual estimates[2], and reduce to a very low figure any changes of sea level, which can be attributed to mountain attraction. It is true that, if we regard the earth as solid, supporting mountains by mere rigidity, "large tracts of country may produce great disturbance of the sea level; but it is at least questionable whether in point of fact they do so[3]". We seem then to be justified in accepting the observed heights of the stations given in the table at page 208 as the true heights above the mean surface of the ocean.

Next with regard to the effect which an elevated tract, supported in hydrostatic equilibrium, would produce upon the vibration number of the pendulum. In the case of a very

[1] See "On the Variations of Gravity," &c., p. 15. (In the second line of that page *for* $(\sigma - \mu)$ *read* $\frac{\sigma - \mu}{\rho}$.)

[2] *E.g.* In Prof. Hull's article in "Geol. Mag.," March, 1888, Dec. III., vol. V., p. 113.

[3] "Clarke's Geodesy," p. 96. Oxford, 1880.

extensive plateau the effect would be *nil*. This arises from the circumstance, that the attractive effect of an infinite plate upon a particle placed above it is independent of its distance from the plate. The diminution of attraction, resulting from the deficiency of density in the root of the plateau, in that case exactly counterbalances the attraction of the plateau itself, so that there will be no residual effect upon the pendulum. It is therefore the form and dimensions of the region to which we must look, to account for any effect which may have been observed. In the article in the "Philosophical Magazine" will be found a calculation for a long parallelepiped, which will be fairly applicable to the case of a station situated as Moré is; and if we introduce into the formula the values which we have assigned to the thickness of the undisturbed crust, and to the densities, it comes out, that the attractive effect of the parallelepiped and its root would be competent to increase the vibration number of the pendulum by 4·15 swings *per diem*; whereas in fact, as shown in column 5, the number of swings to be so accounted for was 2·26. But inasmuch as it was calculated that the visible masses would of themselves have produced 23·45 swings, the real defect was 23·45 – 2·26 or 21·19 swings, whereas the calculated effect, on the hypothesis just made, would have been 23·45 – 4·15 or 19·30; so that, on the rough comparison with the parallelepiped, the theory accounts for 19·3 swings out of 21·2; that is, it errs by 1·9 swings only, which is remarkably little. So that, in this extreme instance of the loftiest station reached, the theory of hydrostatic equilibrium, with the assumed values of the thickness of the crust and the densities, may be considered to give a satisfactory explanation of the phenomena.

A second instance of a calculation of a somewhat different character will be found relating to Kaliana[1], which is a station on the plain south of the Himalayas, about 60 miles from the foot of the range. The agreement with the observed effect is also in this case very close. For the observed consequence of local attraction there was to produce 2·28 swings in defect, whereas the calculation on our hypothesis gives 2·73 in defect.

[1] "On the Variations of Gravity," p. 22.

It will be seen, by referring to the sixth column, that the visible masses ought to have produced 1·25 swings in excess.

Since it did not appear practicable to represent the environment of the two other "very irregularly surrounded" mountain stations, Dehra and Mussoorie, in a geometrical manner, no attempt was made to calculate the attractions at them on the hydrostatic hypothesis. It may therefore be claimed that, in the two instances in which the attempt was made, it succeeded as well as could be expected.

We will conclude the present chapter with some remarks upon the variation of gravity at certain stations in "Peninsular" India.

There are no true mountain-ranges in Peninsular India, the so-called "mountains" being only the escarpments of plateaux which have escaped denudation. "Peninsular India is, in fact, a tableland, worn away by subaerial denudation, and perhaps to a minor extent on its margins by the sea[1]." The Deccan traps are of Lower-Eocene age, covered in places by nummulitic rocks[2]. Their total thickness may be 6000 feet[3]. The horizontality of the flows in these plateaux is remarkable. In considering the bearing of the gravitational phenomena at stations in this part of India, we ought to take its structure into account. No root has been formed by compression during the formation of its hills. As the country became gradually weighted by flow upon flow of the basalts, the crust must have sunk gradually into the magma. The Geological Survey does not appear to have yet mastered the details; but possibly it will be found that the country is faulted, and consequently deep roots will answer rather to low elevations than to high ones, and the equilibrium will be of the tract as a whole, instead of being established within every vertical boundary.

In any case where the attraction of the mass above the sea level may be fairly taken as due to an infinite plain, it appears

[1] "Manual of the Geology of India," by Medlicott and Blanford (Calcutta, 1879), p. v. The contrast between the peninsular and Himalayan regions was strikingly shown by a large model in the Indian Annexe of the Indo-Colonial Exhibition at South Kensington in 1886.

[2] *Ibid.* p. 381. [3] *Ibid.* p. 308.

as has been already shown[1] that the negative attraction of the root would, on the hypothesis of hydrostatic equilibrium being established within every vertical boundary, exactly balance it; and the resulting local attraction at the station ought to be *nil.* Now there are seven stations of the great arc between latitudes 16° and 24° N. which are upon the basalt, viz. from Pahargarh to Kodangal inclusive; and the local attractions, relative to the attraction at sea-level at Punnæ, in swings of the pendulum, range for these stations, as shown in column 5, from $-0{\cdot}79$ to $+1{\cdot}75$, being as below :—

Heights.	Station.	Local attraction relative to Punnæ.
ft.		
1914	Kodangal	+1·00
1946	Damargida	−0·79
1714	Somtana	+0·93
1120	Badgaon	+0·32
1693	Ahmadpur	+0·79
1763	Kalianpur............	+1·75
1641	Pahargarh	−0·42
Mean 1685	....	+0·51

These numbers will be uniformly increased or diminished by any local attraction there may be at Punnæ, which is not situated on an infinite plain, but where proximity to the coast places it under conditions different from those at the stations which we are referring to it.

To appreciate what these differences from zero attraction at the stations imply, we observe that one vibration *per diem* due to local attraction corresponds to about 645 feet of elevation of a plain; and therefore, since the root (supposed of density ρ) displaces a layer of density σ, one vibration in defect caused by it will correspond to $\frac{\rho}{\sigma-\rho}\times 645$, or 6172 feet, *i.e.* to 1·1707 mile of root.

We see, then, that the root at Damargida, where the attrac-

[1] p. 212.

tion is $-0·79$, would be about 0·92 mile too deep for local equilibrium; and at Kalianpur, where it is $+1·75$, it would be 2·046 miles too shallow—these estimates being of course subject to the uncertainty belonging to local attraction at Punnæ.

If we assume the crust at Punnæ to be 25 miles thick, then, the depth for zero attraction of the root at Damargida being 3·527 miles, the actual depth there would be 5·171 and the whole thickness of the crust 30 miles. At Kalianpur, the depth for zero attraction being 3·195 miles, the actual depth would be 1·149 and the whole thickness 26 miles. These two instances are the greatest variations that occur throughout 8 degrees of latitude, and there are but two others of similar amount among the sixteen stations between Kaliana and Punnæ.

Although these varying local attractions at the several stations make it clear that this region is not in hydrostatic equilibrium everywhere locally, nevertheless, the relative attractions being at some stations positive and at others negative, it is quite possible that it may be as a whole supported in that manner, because we do not know how large the areas may be which show positive or negative relative attraction under column 5. The mean of the local attraction relative to Punnæ is only 0·51 of a swing *per diem*; and if we omit the station Kalianpur, which seems to be subject to some peculiar influence (perhaps of a volcanic neck of basalt), this is reduced to 0·30 of a swing, the result does not seem to discredit the hypothesis that there is a distribution of matter not far differing from what would accord with equilibrium for the region as a whole. The attractions at these peninsular stations give no information about the *mean* thickness of the crust, assumed at 25 miles, because, in the case of an infinite plain, the terms involving it (k) disappear from the formula.

On the whole, it is apparent that the bottom of the crust is here irregular, and does not locally correspond for equilibrium with the surface-contour. In a basaltic region, as already remarked, this would seem natural; for there will have been no compressing action tending to produce downward bulges corresponding to the elevated tracts, as would be the case in

a mountain-chain; so that any downward projection into the magma (*i.e.* root) will be due simply to the local depression of the crust, owing to its having become overweighted at the top, while rigidity of the crust, never crushed as in a mountain-chain, would extend the depression laterally and diminish it vertically. It is possible that the considerable local thickenings and thinnings of the crust, which appear to occur at a few places in this region, may be due to faults of large throw, such as are not unknown in countries where there has been much out-pouring of basalts.

It is submitted that, considering the complexity of the subject, the results detailed in this chapter are fairly confirmatory of the theory, that elevated tracts are supported in hydrostatic equilibrium upon a yielding substratum; and also that the values of the densities, which have been assumed, are not far from the truth, and to a less extent they confirm the assumption, that the thickness of the crust at the sea coast has been approximately rightly estimated at 25 miles[1].

[1] M. Faye has an article, "Sur la Constitution de la Croute Terrestre," in "Comptes Rendus" for March 22, 1886, which will be further referred to in Chapter XVIII. He there discusses pendulum-observations at island and continental stations, the latter with especial reference to the Indian observations. Respecting the excess of gravity found at island stations, he comes to a conclusion similar to that formerly arrived at by Pratt in reference to Minicoy ("Figure of the Earth," 4th ed., Art. 74). With regard to continents, his views likewise agree generally with those advanced by Pratt; see in particular Pratt's Art. 192.

CHAPTER XVI.

THE REVELATIONS OF THE THERMOMETER.

Recapitulation of results of the two preceding chapters—Roots of mountains should be revealed by phenomena of underground temperature—Conditions upon which the mean rate of increase of temperature will depend—Rate greater in plains and less in mountains—Dr Stapff—His observations on temperature of rocks in St Gothard tunnel—His explanation of the smallness of the rate—Why not satisfactory—Effect of convexity of mountain upon the rate—The rate may be considered uniform above—The conclusion from the premises is that the mountain has a root—The rate may be regarded as nearly uniform throughout—General method of determining the thickness of the crust at the sea-level, and the melting temperature, by comparing the rate beneath the mountain with the usual rate—Application to St Gothard—Thickness of crust at sea-level and melting temperature deduced numerically from the data—The like for Mont Cenis—Results confirmatory of previous conclusions.

WE have seen in the two preceding chapters that certain results of geodesy agree well with the conclusion arrived at in the thirteenth chapter, that mountains have "roots," that is that there is, for most regions elevated above the mean level of the crust, a corresponding downward protrusion of the lighter material of the crust into the heavier substratum. This is in substance the same thing which Herschel expressed by saying that, "the force by which continents are sustained is one of tumefaction[1]." According to our theory these roots will consist of the lighter material of the crust, in an unmelted state, projecting into a heavier molten fluid. The amount of this

[1] See p. 125.

projection may be roughly determined by the relative densities of the crust and fluid, in the same way as if the crust floated without constraint in equilibrium. For although, as we have argued in the thirteenth chapter, the partial rigidity of the crust will render this assumption in strictness incorrect, yet the deviations from accuracy consequent upon it cannot amount to anything considerable when compared to the whole thickness; and this will be especially true in the neighbourhood of the chains, near which the centre of gravity of a disturbed tract will lie; and which will therefore be least depressed or elevated by any slight tilting action.

If this be a true statement of the case, then the existence of these roots ought to be revealed in another and entirely distinct manner, namely by the phenomena of underground temperature. Where the roots of the mountains are in contact with the molten fluid they must be at the same temperature and the like will be true of the bottom of the mean crust. At all localities alike we shall therefore have, maintained at the bottom of the solid crust, sensibly the melting temperature of the materials of which the crust is there composed, modified perhaps by the difference of pressure at their different depths, which we need not for the present purpose consider.

Now it is evident that, if the two surfaces of a plate of any substance are maintained long enough at two different constant temperatures, the mean rate of increase of temperature within the plate will depend solely upon its thickness irrespectively of its conductivity. The mean rate therefore of increase of temperature within the crust, at any locality where we may suppose these conditions approximately fulfilled, will depend upon the difference of the temperatures of its upper and under surfaces at that locality, and on its thickness, and not upon the conductivity of the material, nor upon the absolute temperatures. For although contortions in the bedding, or difference of composition, may affect the rate as between one level and another at that locality, still they will not affect the mean rate. Nor yet need such geological accidents be considered in comparing the mean rate at one place with that at another. The result is that the mean underground rates at all places where

the thickness is the same, and consequently the height above the sea the same and where the mean annual surface temperature is the same, ought to be equal, irrespectively of the nature of the rocks, provided only that the temperature at the bottom of the crust is the same.

But if on the other hand the heights of two localities be different, the mean rates there ought to differ, not in inverse proportion to the heights above the sea, but approximately in inverse proportion to the total thicknesses of the crust, including the roots down to the molten substratum. Is there any reason to believe that this is the case? If we find it to be so, we shall have a presumption in favour of our theory, not only that there is a downward protuberance of lighter material, as already shown to be corroborated by observations with the plumb-line and by pendulum, but that this protuberance consists of unmelted crust projecting into molten liquid.

Of course the above reasoning requires the state of temperature to be sensibly permanent, and that a sufficient time should have elapsed, since the mountains were elevated, for the heat brought up from below along with the uplifted matter to have become so far dissipated, that the flow of heat through the crust has become steady.

Mallet, in his paper on Volcanic energy[1], refers to a dissertation, which he considers "the most complete and valuable collection and discussion of all the observations on record up to June, 1836," by A. Vrolik, who, he states, "believes it proved, that in general the rate of increment is greater in plains and valleys than in mountains."

The great engineering achievements of late years have afforded exceptional facilities for testing this question; and the above statement has been found to be strictly true. An extensive series of observations was carried out more especially in connection with the construction of the tunnel through the Alps at Mont St Gothard, by Dr F. M. Stapff, geological engineer

[1] "Phil. Trans. Roy. Soc.," vol. CLXIII., p. 158.
The title of the dissertation is "Disputatio physica Inauguralis de Calore telluris infra superficiem augescente." 4to., 101 pages. Müller, Amsterdam, 1836.

to the company; and very interesting results were obtained, which are recorded in his published papers[1].

Dr Stapff points out that the rate of increase of temperature beneath the mountain differs from that beneath the plane surface, and gives it as his opinion that the influence which the mass of the mountain superimposed, and free on each side, exercises on the temperature of the rocks, is very different from that which an envelope would exercise, formed of the crust of the earth of a thickness equal to the height of this same mass[2]. This conclusion is ostensibly corroborated by the fact, that the augmentation of temperature is observed to be more rapid beneath the hollows and flat spaces of the open surface above the tunnel than beneath the summits. But although this is no doubt true, nevertheless it may be shown that the contour of the mountain cannot account for a general rate of increase of temperature beneath it so much slower than the usual one. The mean rate at Mt St Gothard for the whole tunnel is found by Dr Stapff to be 0·0206° C. per metre of descent, which is about 1/88 degree F. per foot[3]. This is little more than half the usual rate of increase. The rate was found to exceed this mean on the north more often than on the south side of the mountain, This was attributed to local causes inherent in the rock itself[4].

[1] "Studien über die Wärmevertheilung im Gotthard." Bern, 1877. "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre sur la possibilité de construction des Tunnels dans les hautes Montagnes. Première partie, 1879. Do. deuxième partie, 1880. Revue universelle des Mines, etc. Annuaire de l'Association des Ingénieurs." Paris, 9, Rue des Saints-Pères. Londres, 5, Bouverie Street. Liége, 24, Rue d'Archis. "Repartition de la température dans le grand tunnel du St Gothard. Annexe XIV au volume VIII des rapports trimestriels du Conseil fédéral sur la marche des travaux du chemin de fer du St Gothard (rapport N°. 30)." 1880.

[2] "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre," &c. Deuxième partie, p. 2.

[3] The factor which reduces the former to the latter system of units is 0·548. But if it is wished to obtain the number of feet of descent, which corresponds to 1° F., subtract the logarithm of the cent. met. rate from 0·2606013, and it gives the logarithm of the number of feet required.

[4] Dr Stapff attributed the excess of temperature in the Northern half of the tunnel under the Plain of Andermatt to warm springs. (See note, p. 6, *loc. cit.*) Prof. Prestwich thinks it may be attributed to mechanical action arising from friction and compression during the comparatively recent elevation of this part

To estimate roughly the effect which the contour of the mountain would produce in lowering the rate of increase, we may argue thus. As we ascend in the atmosphere the temperature decreases. The temperature of the summit of the mountain is found to be zero centigrade, or the freezing-point. Calculate then the altitudes at which the observed rates at equal horizontal distances along the tunnel would bring us to the freezing-point within the rock supposing it extended to a sufficient height, and draw a curve through these points, which will touch the summit of the mountain. This curve will define what may be called the effective profile of the mountain, that is to say, the form of outer surface, which would give the same temperatures within the mountain as actually exist, were the temperature of the surface uniform. It will be seen that the contour so obtained is very considerably less convex than the actual contour. This shows that the real effect of the convexity of the surface upon the internal temperatures cannot be so great as at first sight it might appear likely to be.

The isogeotherms within the mountain will evidently consist of a family of convex surfaces corresponding to the external effective profile, but decreasing in convexity with increasing depth. Eventually a mean plane horizontal isogeotherm will be reached. But it does not follow that the isogeotherms will continue to be planes beneath it.

Let S be the area of one of these surfaces within the mountain, through which the same heat passes in a given interval of time as that which crosses the area A of the plane isogeotherm in the same interval. Let n be measured along the normal drawn to a point in the surface S, v being the temperature at that point, and κ the conductivity. Then the flow of heat across the elementary area dS will be

$$\kappa \frac{dv}{dn} dS.$$

of the chain. It may however be remarked that, looking at Dr Stapff's geological section (which has been reproduced by Prof. Prestwich in the "Proc. Roy. Soc.," no. 246, 1886, p. 46; and in his "Geology," vol. I., p. 304, plate, 1888), where warm springs and an abnormally high temperature occur, there are deep synclinals in the strata. This is the arrangement calculated naturally to produce warm springs, as in the case of the Bath waters. See p. 20.

Properly speaking $\frac{dv}{dn}$ will vary from point to point of the isogeotherm. But if we consider it to be the average rate along the normal at every point of a given isogeotherm, then the flow across the whole of that surface will be

$$\kappa \frac{dv}{dn} S.$$

But the same flow of heat crosses the plane area A, where let us call r' the average rate of increase of temperature in descending; wherefore the whole flow there will be $\kappa r' A$.

Hence if $\frac{dv}{dn}$ is the mean rate, we must have

$$\frac{dv}{dn} S = r' A,$$

$$\text{and } \therefore \frac{dv}{dn} = \frac{A}{S} r'.$$

This gives the proportion according to which the average rate along any normal to an isogeotherm is diminished in consequence of the convexity of its surface. This rate will clearly become more and more nearly equal to that through the plane isogeotherm, as that is approached.

Now the whole length of the St Gothard tunnel is 14920 metres, and the greatest height of the mountain above it is 1734 metres. It is therefore obvious that the ratio $\frac{A}{S}$ must be nearly one of equality. Indeed, if we suppose the contour of the surface of the mountain to be a segment of a circular cylinder, whose chord is the length of the tunnel, we shall find that the value of $\frac{A}{S}$ for the outer surface, where that ratio will differ most from equality, will be 0·965. And consequently, if we suppose that the temperature of this surface is everywhere 0° C. (an unfavourable supposition), then

$$\frac{dv}{dn} = 0{\cdot}965 r'.$$

This shows that the convexity of the mountain can only slightly affect the rate along the vertical anywhere, and in the central

part, where $\frac{dv}{dn}$ becomes $\frac{dv}{dx}$, scarcely at all. We are therefore at liberty to regard the rate in the central part of the mountain as very nearly constant and equal to r'.

Now the temperature at the upper surface being 0° C., if h be the height of the mountain above the tunnel, and t the temperature of the rock in the centre of the tunnel, the rate

$$r' = \frac{t}{h}.$$

But at the depth where the plane isogeotherm is encountered the temperature beneath the mountain must equal that of the neighbouring crust at the same level, because the isogeotherm is plane. It is obvious therefore from geometry, the rates beneath the mountain and at any place in the neighbourhood being both of them uniform yet not the same, that this plane isogeotherm cannot be elsewhere than at the bottom of the crust, unless the isogeotherms beneath the mountain become concave upwards after the plane isogeotherm is passed. Suppose then the former to be the case, and that k is the mean average thickness of the crust, and k' its thickness beneath the mountain; at which respective depths (if the isogeotherms beneath the mountain do not become concave) the temperatures become equal. Then we must have,

$$\frac{k}{k'} = \frac{r'}{r}.$$

But observation shows that r' is about $r/2$,

$$\therefore k' = 2k;$$

or the height of the mountain above the mean level of the crust would in such case be equal to the thickness of the crust. This would make the crust far thinner than it can be admitted to be; and therefore shows that the plane isogeotherm is not at the bottom of the crust, and that the isogeotherms below the plane one must have a convexity downwards, answering to their convexity upwards in the upper portion. There must therefore be a protuberance of solid crust below corresponding to the elevation of the mass above. We see then that we have here

an additional proof of the existence of roots to the mountains derived from thermal phenomena.

The downward protuberance will be much greater than the upward elevation, and therefore we perhaps ought not to assume that the extreme value of the ratio of $A : S$ will be so nearly one of equality below the plane isogeotherm as above it. But it is probable that the downward protuberance, although it may be ten times as deep as the other is high, will be very much wider, because the St Gothard mountain is only a remnant, carved by subaerial denudation out of a much more extended elevated tract, with which the root was originally conterminous: and whatever, if any, denudation, or melting off analogous to it, the roots of the mountain range may have undergone, it is not likely that their contour should be reduced to the semblance of inverted mountains and alternating valleys, like those which characterise the upper surface of an Alpine region. The ratio of $A : S$ may therefore be still regarded as nearly one of equality even in the lower portions.

If we are thus at liberty to regard the rate beneath the mountain as approximately uniform, it is possible to estimate from the data the thickness of the crust at a place on the sea-level, and likewise the melting temperature at the bottom of it, if we assume a ratio for the densities of the crust and fluid substratum. For generally let

$c =$ the depth of a point in the crust from the surface,

$a =$ the temperature at that depth,

$b =$ the temperature at the surface,

$r =$ the rate of increase of temperature in descending.

Then we have the general relation,

$$\frac{a-b}{c} = r \qquad \text{(1)}.$$

If v be the rock-temperature in the tunnel, b' that at the top of the mountain, m the height of the mountain above the tunnel, r' the rate, we have in like manner

$$\frac{v-b'}{m} = r' \qquad \text{(2)},$$

whence r' may be obtained from the observed data.

Now suppose equation (1) to refer to some place A, anywhere near the sea-level, where the total thickness of the crust is c, and the rate the usual one r. Then the temperature at the bottom of the crust at A will be given by

$$a = cr + b.$$

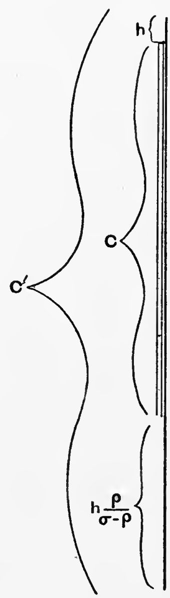
Similarly, if c' be the thickness of the crust at the mountain, the temperatures at the bottom of the crust being the same at both places because it is the melting temperature,

$$a = c'r' + b'.$$

Consequently

$$cr + b = c'r' + b' \quad \text{......................(3).}$$

In this equation r, r', b, b' are known, and therefore, if we can obtain another relation between c and c', we shall be able to find the thicknesses of the crust at the place A on the sea-level and at the mountain. If we are at liberty to apply the conditions of hydrostatic equilibrium, such a relation is easily obtained, as shown on the figure, from which it appears that,



$$c' = h + c + h\frac{\rho}{\sigma - \rho},$$

$$= c + h\frac{\sigma}{\sigma - \rho}.$$

But from (3) we have

$$c' = \frac{r}{r'}c + \frac{b - b'}{r'},$$

whence

$$c = \frac{r'}{r - r'}\frac{\sigma}{\sigma - \rho}h - \frac{b - b'}{r - r'} \quad \text{......(4).}$$

If we assume a value for the ratio $\frac{\rho}{\sigma}$, this expression gives the thickness of the crust at a place A at the sea-level, where the surface temperature is b, and the rate r. The place ought not to be in the mountainous region, in order that the rate may not be affected by the additional thickness of crust beneath such a region.

Let us now apply equation (4) to find the thickness of the earth's crust at a place at the sea-level, from the data obtained from the St Gothard tunnel. In Table VIII. of Dr Stapff's paper[1], the rate at the exact middle of the tunnel is not given, but from the plate which illustrates his "Répartition de la Température" it can be found. The height of the mountain above the sea over the middle point is there seen to be 2839 metres, and the altitude of the tunnel itself 1127 metres. The temperature of the rock in the tunnel is 30°·8 C., and of the ground, at the summit of the mountain above, 0° C. Hence the rate is 30·8/1712 degree C. per metre, which is nearly 1/102 degree F. per foot. We will first assume the rate at the place A at the sea-level to be 1/51 degree F. per foot, and the mean surface temperature there to be 50° F. Also if we take, as we have previously done, the ratio of $\rho : \sigma$ as that of granite to basalt, viz. 2·68 : 2·96, we find that $\frac{\sigma}{\sigma - \rho} = 10 \cdot 57$. Then, expressing the altitudes in feet, we have for the values of our symbols,

$$r = \frac{1}{51}, \quad r' = \frac{1}{102},$$

$$\frac{r'}{r - r'} = 1, \quad \frac{\sigma}{\sigma - \rho} = 10 \cdot 57, \quad h = 9315,$$

$$b - b' = 18, \quad \frac{1}{r - r'} = 102.$$

The thickness of the crust at the place A then comes out,

$$c = 96623 \text{ feet} = 18 \cdot 3 \text{ miles}.$$

And for the melting temperature,

$$\begin{aligned} a &= cr + b \\ &= 1944^\circ \text{ F}. \end{aligned}$$

We also find for the thickness of the crust at the mountain,

$$\begin{aligned} c' &= \frac{a - b'}{r'} = 1912 \times 102 \text{ feet} \\ &= 36 \cdot 9 \text{ miles}. \end{aligned}$$

[1] "Étude de l'Influence de la Chaleur de l'Intérieur de la Terre, &c." Deuxième partie, p. 7. See note, p. 221.

If however we assume 1/60 degree F. per foot as the rate of increment of temperature at the place on the sea-level, instead of 1/51, we then obtain for the thickness of the crust there,

$$c = 138032 \text{ feet}$$
$$= 26{\cdot}1 \text{ miles.}$$

And for the melting temperature

$$a = 2350^\circ \text{ F.}$$

And for the thickness of the crust at the mountain

$$c' = 236436 \text{ feet}$$
$$= 44{\cdot}8 \text{ miles.}$$

The observations which were made upon the temperature of the rocks in the Mont Cenis[1] tunnel, were less systematic than in the tunnel through Mont S. Gothard. The highest temperature recorded was 85° F. at a distance of 21000 feet from the southern opening, while the whole length of the tunnel was 40094 feet; so that the highest temperature occurred at about the middle of the tunnel. At that point the elevation of the tunnel above the sea was about 4391 feet, and the greatest height of the mass of the Alps over the tunnel was 5307 feet. If we assume the temperature of the rock at the surface to be the freezing temperature, as in the case of S. Gothard, the above data give $r' = 1/100$. And if we take the rate at a place on the sea-level to be 1/51, and the temperature there 50° F., these data, when introduced into our equation, give

$$c = 19{\cdot}8 \text{ miles,}$$

and a the melting temperature $= 2105^\circ$ F.

But if we take 1/60 as the rate at a place on the sea-level, then

$$c = 28{\cdot}6 \text{ miles,}$$

and $$a = 2568^\circ \text{ F.}$$

The rates of increase of temperature have been found to vary so much in different localities, that the rates at these two tunnels, though so nearly the same (and yet they are

[1] "Nature," vol. IV., p. 36, p. 415, and p. 434.

distant apart more than 130 miles), may differ somewhat from what might prove to be the average, were a greater number of mountains to be pierced. We may however feel assured that the rates determined represent pretty accurately the true mean rates at those places, because the amount of cover is so great, that the average is obtained for a great depth; a depth greater than has been reached in almost any other artificial excavation.

The results obtained in the present chapter are decidedly corroborative of the views put forward in the thirteenth chapter respecting the configuration of a section across a disturbed tract. For it is not easy to see on what hypothesis the conclusion regarding the downward convexity of the isogeotherms beneath a mountain can be explained, except on that of a protuberance of unmelted into melted matter. Indeed the existence of such roots to the mountain appears to be proved as fully by the argument from temperature, as by that from attraction.

Again, the melting temperature at which we have arrived without making any assumption about the temperature at which the rocks would melt, agrees very nearly with that of cast iron[1], which is about that which has been assumed by the older geologists as the probable temperature below the solid crust, whence the usual estimate for the thickness of the crust has been derived of about 20 miles which would be sufficient to satisfy all the requirements of their science[2].

But there is reason to think that the crust may be somewhat thicker, and consequently the temperature somewhat higher than we have just estimated them, even on the showing of our own formulæ, and that for the following reason. The value of c in equation (4) is increased if the ratio of $\sigma : \rho$ is made more nearly one of equality. We have taken this ratio to be that of basalt to granite, viz $2{\cdot}96 : 2{\cdot}68$; and the reason why we have done so is because the denser eruptive rocks

[1] "The temperature of that region of the furnace [iron] where complete liquefaction takes place is generally supposed to be a little above 1500° C.," or 2732° F. Sir I. Lowthian Bell, "On the occlusion of gaseous matter." "Journal of Iron and Steel Institute," no. II., 1881, p. 3.

Dana considers the temperature of the lava in the volcano of Kilauea to be "2000° F. or more" "Amer. Journ. Sc.," 1888, p. 286.

[2] Prestwich, "Geology," vol. II., p. 544, 1888.

are of the basaltic type, so that we may reasonably infer that the heavier fluid substratum is, as regards mineral character, a basic magma. But it by no means follows that, while not yet erupted, it should be as dense as basalt. Indeed the probability is in the other direction; because if, as we believe, it is in a condition of igneo-aqueous solution, that circumstance as well as its high temperature may diminish its density.

There is yet another reason, though perhaps a less cogent one, to lead us to suppose that we are taking the density of the substratum too great, if we assume it equal to that of basalt; for it has been already pointed out[1] that, according to Laplace's law, the density (2·96) of basalt would be found at the depth of 100 miles, which is much more than that, at which we should expect the temperature of fusion to commence. It seems therefore probable that the value of σ is nearer to that of ρ than it has been assumed to be, and the resulting value for the thickness of the crust greater, and the temperature higher than those found; while the downward protuberance below the mountains will be also greater than estimated[2].

It is probable that the water, which is believed to be present, will render the rocks fusible at a lower temperature, and increase the liquidity of the magma.

Again we see, from the difference in the results for the thickness and melting temperature according as we take 1/51 or 1/60 for the average rate, that the question we are dealing with is a delicate one, at the same time depending upon data about which we have no very precise knowledge. The thickness may therefore differ from the above results by a few miles, and the temperature by a few degrees either way. But if we are led to seek relief from the somewhat unexpectedly small values, which we have obtained by using the rate 1/51, for the thickness of the crust (because it will be recollected we have previously assumed it at 25 miles[3]) and of the melting temperature, it seems on the whole that it is to be found rather in assuming a lesser density for the substratum, than in assuming

[1] See note, p. 168.
[2] See below, p. 248.
[3] Chap. XIII., p. 178.

the lower value 1/60 for the temperature rate[1]. But with every allowance made for some uncertainty, it must be admitted that the results at which we have arrived are singularly near what on other grounds we may expect to be the truth, while no gratuitous assumption has to be made in obtaining them.

It is also not unimportant that the two lines of argument in the preceding and present chapters, so diverse in their characters and yet pointing in a like direction, are drawn from observations made in regions as far apart as the Himalayas and the Alps; the conclusion from both being the same, namely, that there is a protrusion of the lighter material of the crust into the denser subjacent fluid beneath the mountains in both these regions.

[1] For reasons for supposing 1/51 to be rather below than above the average rate, see p. 20.

CHAPTER XVII.

THE SUB-OCEANIC CRUST.

The crust once beneath deep oceans may never come under observation—Opinion that oceanic and continental areas are permanent—Zones of depth—Problem proposed to estimate the thickness and density of the sub-oceanic crust—Gravity over a spherical terraqueous globe however constituted will be constant—Definition of a cap-sector—Attractive effects of a shell consisting of layers of varying densities together with a nucleus assimilated to that of the earth—Thickness and density of the sub-oceanic crust compared with those of the continental crust upon several hypotheses—A suitable hypothesis found—Sub-oceanic crust probably not more than about 25 miles thick—Consists of two layers of different densities—The lower layer the thinner and more dense—Convection currents in the substratum ascend beneath the oceans—Mean density of the crust there greater than beneath the land—The crust may be sinking where the depths are abysmal—Earthquakes of Japan—Plateaux of mid-ocean—Collection of water in ocean basins—Their permanence—Effect of currents in modifying the calculations in the preceding chapter.

We have in the three preceding chapters discussed the manner in which continental masses elevated above the sea level may be supported in approximate hydrostatic equilibrium, and have shown that the theory is consistent with certain apparent anomalies in the phenomena of gravity and of temperature. In the present chapter we shall direct our attention to the sub-oceanic crust. We cannot explore this directly, but seeing that most of the strata of the exposed portions of the earth's surface have been deposited beneath seas, it might be natural to suppose that the sub-oceanic crust is generally analogous to the continental, which is so largely composed of

old sea bottoms uplifted. This however might be a hasty inference, especially when we consider that most of the strata which we meet with, enormously thick as some of them are, nevertheless show signs of having been laid down in not very deep water; while at the same time it is hardly likely that sediment can have been spread out very far from the lands, from whose disintegration it has been derived.

It is not improbable therefore that the crust of the earth once beneath the deep oceans, may never come under observation, and may be very unlike the marine strata with which we are ordinarily conversant. Indeed it is the conviction of a large number of geologists[1], whose judgment is of weight on such a point, that the continents have always occupied the positions which they now occupy: by which it must be understood, that they have oscillated about their present positions, sometimes more extended on one side and sometimes on another; but that the great oceanic areas and the great continental areas have never, within times to which geological records go back, interchanged places. If this be a true statement of the case, then it may be asserted that we know nothing about the geological constitution of the earth's crust beneath the great oceans.

Referring to works[2] which have been published since the voyage of the *Challenger*, we find the oceanic area divided into five portions, whose mean depths are taken to be,

(1) from 1 to 2 miles,
(2) from 2 to 3 miles,
(3) from 3 to 4 miles,
(4) from 4 to 5 miles,
(5) above 5 miles.

The first may be taken as extensions of the elevations which have produced the continents. The second are sometimes connected with and prolongations of the first. The third which occupy the larger portion of the oceanic area we will regard

[1] See a resume of the history of this theory in a short letter by Dana, "Nature," vol. XXIII., p. 410.

[2] For example, "Letts's popular Atlas" maps 3 and 4. See also "Thalassa" by J. J. Wild. Ch. I. Marcus Ward, 1877.

as indicating the mean upper level of the crust. The fourth and fifth will then be depressions below it[1].

We now propose to seek estimates of the thickness and density of the crust beneath oceans from mathematical considerations.

Let us imagine the earth to be a sphere, (that is, we set aside the rotational effects) and that its surface is formed of flat land and water, all being at the mean level. But in fact, though we make this supposition for simplicity, if there be any flat tract of land at the ocean level it will suffice for our purpose. Our symbols are then assumed as follow:

a = radius of the sphere,
c = the thickness of the crust at the sea board,
ρ = its density there, taken, as hitherto, at 2·68,
k = the thickness of the sub-oceanic crust at a place where the depth of the ocean is δ,
δ = the depth of the ocean at any place,
ρ' = the density of the sub-oceanic crust at that place,
μ = the density of the ocean,
σ = the density of the liquid substratum, taken constant to the depth to which the rigid crust anywhere extends; and as hitherto assumed, equal to 2·96.

If the earth be taken as spherical, then gravity will be the same everywhere on its surface, however the matter of which it consists may be distributed; for Professor Stokes has proved that, if the earth is a spheroid of equilibrium of small ellipticity, then, however the matter may be distributed, Clairaut's theorem holds good that,

$$g = \frac{E}{a^2}\{1 + (\tfrac{5}{2}m - \epsilon)\sin^2 l\}.$$

But if the earth is regarded as a sphere, there is no centri-

[1] "Contrary to the ideas formerly entertained of the enormous depths of the ocean the soundings of H.M.S. 'Challenger,' S.M.S. 'Gazelle,' and of the U.S.S. 'Tuscarora' and 'Gettysburg' indicate that depths of five miles or over 4000 fathoms are seldom met with and are as exceptional as heights of the same amount on land." Wild's "Thalassa," p. 14, 1877.

fugal force, and no ellipticity, and then the above expression gives

$$g = \frac{E}{a^2};$$

or gravity is constant all over the surface.

In order to compare the thickness and density of the crust beneath the ocean at a place where δ is the depth of the water with the thickness and density at the sea-board, we may make use of the following considerations of attraction. Regarding the earth as a sphere, a "spherical cap" is defined as the part of a spherical shell bounded by a right cone, whose vertex is the centre of the sphere. And a "cap-sector" is the portion of such a cap intercepted between two planes passing through the axis of the cone, and inclined to each other at the sector angle α.

Suppose a cap-sector whose apex is A to subtend at the centre an angle θ. Let its thickness be t_1 and its density τ_1, and a the radius of the sphere. Then, expanding in terms of $\frac{t_1}{a}$, the vertical attraction of this sector upon a particle at A will be of the form

$$\alpha\tau_1 \left\{t_1 f(\theta) + \frac{t_1^2}{a}\phi(\theta) + \frac{t_1^3}{a^2}\psi(\theta) + \&c.\right\},$$

because it must be of dimension L, and must vanish with t_1.

Let this be overlapped by another cap-sector of greater thickness t_2 and of density τ_2. Then the attraction of the composite cap-sector will be

$$\alpha \left\{(\tau_1 t_1 + \tau_2 t_2) f(\theta) + \left(\tau_1 \frac{t_1^2}{a} + \tau_2 \frac{t_2^2}{a}\right)\phi(\theta) + \&c.\right\};$$

the density of the layer common to the two simple sectors being $\tau_1 + \tau_2$. And similarly if there are m layers in the cap-sector, their combined vertical attraction at its apex will be

$$\alpha\left\{\Sigma_m (\tau t) f(\theta) + \Sigma_m \left(\tau\frac{t^2}{a}\right)\phi(\theta) + \&c.\right\};$$

the density of any particular layer being the sum of the densities of the layers overlapping at that depth.

Let there be another composite cap-sector made up of n

different thicknesses t_1' &c., of different respective densities τ_1' &c., a and θ remaining unaltered, and t_1' &c. and τ_1' &c. being so assumed that the vertical component of its attraction on a particle at A is the same as that of the former composite cap-sector.

Then we shall have

$$a\left\{\Sigma_m(\tau t)\,f(\theta)+\Sigma_m\left(\tau\frac{t^2}{a}\right)\phi(\theta)+\text{\&c.}\right\}$$

$$=a\left\{\Sigma_n(\tau' t')\,f(\theta)+\Sigma_n\left(\tau'\frac{t'^2}{a}\right)\phi(\theta)+\text{\&c.}\right\}\text{.........(1).}$$

This will be satisfied to the order of approximation $1/a$ independently of the value of θ, if $\Sigma_m(\tau t)=\Sigma_n(\tau' t')$ and $\Sigma_m(\tau t^2)=\Sigma_n(\tau' t'^2)$.

These two conditions would make the masses of the two composite cap-sectors equal to the same degree of approximation, because these masses are respectively proportional to

$$\Sigma_m(\tau t)-\frac{1}{a}\Sigma_m(\tau t^2)+\frac{1}{3a^2}\Sigma_m(\tau t^3)$$

and $$\Sigma_n(\tau' t')-\frac{1}{a}\Sigma_n(\tau' t'^2)+\frac{1}{3a^2}\Sigma_n(\tau' t'^3).$$

Now if equation (1) be satisfied independently of the value of θ, the vertical attraction at A of two composite cap-sectors which subtend a larger angle χ at the centre will be also equal. It follows that the vertical attraction at A of the portion of the one, included between the angles χ and θ, will be equal to that of the portion of the other included between the same angles, the masses of these portions being necessarily equal. This will be true whether a and $\chi-\theta$ be finite or infinitesimal.

We see then that it will be possible to construct an entire shell, whose outer surface is spherical, out of patches of any size and shape, such that the vertical attraction at any selected place will be the same as that of a uniform shell constituted as any one of the patches is; provided that we have $\Sigma(\tau t)$ and $\Sigma(\tau t^2)$ constant all over the shell. If we now make the total thickness of each patch the same, the inner surface of the shell will be likewise spherical and concentric with the outer.

We may go on to suppose the interior of the shell to be filled by a spherical nucleus consisting of concentric layers each of uniform density throughout; and we shall then have a composite spherical body whose vertical attraction will be uniform over its surface, and which will therefore fulfil two of the conditions which we have to realise in the case of the earth, viz. sphericity and equality of attraction. And it does not appear easy to conceive how these conditions could be fulfilled by any arrangement of the layers different from those which we have indicated. It may also be observed that, since the mass of the whole earth is constant and the mass of the nucleus constant, their difference, which is the mass of the shell under consideration, will be a constant quantity. Now from our arrangement of densities and thicknesses it follows, that an entire shell constituted as any one of the patches is may be substituted for the system of patches, and that the mass of any elementary frustrum of one patch is equal to that of any other. This therefore is in accordance with the necessary constancy of mass of the whole shell.

It will be observed that the above reasoning does not make any supposition as to whether the layers, of which the patches are composed, are solid or liquid. The results obtained will therefore not involve any considerations of hydrostatic equilibrium, layers of the ocean, solid crust, and liquid substratum, being indifferently included in the patches.

In adapting our equations, it matters not which part of the crust we suppose to project deeper into the substratum. Let us suppose it to be the crust beneath the ocean, trusting to our result to discover whether that is really the case.

Now let t be the total thickness from the outer surface to the bottom of the crust at the place where it dips deepest into the substratum. Then our two conditions take the form

$$\tau_1 t_1 + \tau_2 t_2 + \tau_3 t = \tau_1' t_1' + \tau_2' t,$$

and

$$\tau_1 t_1^2 + \tau_2 t_2^2 + \tau_3 t^2 = \tau_1' t_1'^2 + \tau_2' t^2.$$

In order to make this notation correspond with that on page 234 we must write $\mu - \rho'$ for τ_1, $\rho' - \sigma$ for τ_2, σ for τ_3,

$\rho - \sigma$ for τ_1', σ for τ_2', δ for t_1, $\delta + k$ for t_2, and c for t_1'. The equations then become

$$(\mu - \rho')\,\delta + (\rho' - \sigma)(\delta + k) + \sigma t = (\rho - \sigma)\,c + \sigma t,$$

and $$(\mu - \rho')\,\delta^2 + (\rho' - \sigma)(\delta + k)^2 + \sigma t^2 = (\rho - \sigma)\,c^2 + \sigma t^2.$$

Cancelling the terms in t and transposing, we have

$$(\sigma - \mu)\,\delta - (\sigma - \rho')\,k = (\sigma - \rho)\,c \quad \ldots\ldots\ldots\ldots(2),$$

and $$(\sigma - \mu)\,\delta^2 - (\sigma - \rho')\,k\,(2\delta + k) = (\sigma - \rho)\,c^2 \quad \ldots\ldots(3).$$

Since μ, ρ, σ and c, are supposed known, these two equations will enable us to find k and ρ' if δ be given, that is, to find the thickness and density of the sub-oceanic crust below any given place in the ocean.

If we multiply equation (2) by $2\delta + k$, and subtract equation (3) from the product, we have

$$(\sigma - \mu)\,\delta\,(\delta + k) = (\sigma - \rho)\,c\,(2\delta + k) - (\sigma - \rho)\,c^2,$$

$$\therefore\ (\sigma - \mu)\,\delta^2 + (\sigma - \rho)(c^2 - 2c\delta) = k\,\{(\sigma - \rho)\,c - (\sigma - \mu)\,\delta\},$$

whence $$k = \frac{c^2 - 2c\delta + \dfrac{\sigma - \mu}{\sigma - \rho}\,\delta^2}{c - \dfrac{\sigma - \mu}{\sigma - \rho}\,\delta}.$$

To obtain ρ' we have

$$\{(\rho' - \mu)\,\delta - (\sigma - \rho)\,c\}^2 = (\sigma - \rho')^2\,(\delta + k)^2$$
$$= (\sigma - \rho')\,\{(\sigma - \rho)\,c^2 - (\rho' - \mu)\,\delta^2\},$$

which gives

$$\rho' = \frac{\rho c^2 - 2\mu c\delta + \mu\,\dfrac{\sigma - \mu}{\sigma - \rho}\,\delta^2}{c^2 - 2c\delta + \dfrac{\sigma - \mu}{\sigma - \rho}\,\delta^2}.$$

It may be mentioned that equation (2) is the same as we should obtain for the condition of hydrostatic equilibrium upon the assumption that gravity is constant throughout the depth considered[1].

In order that the density (ρ') of the sub-oceanic crust may be less than that (σ) of the substratum we must have

$$c > \frac{\sigma - \mu}{\sigma - \rho}\,\delta.$$

[1] See First Edition, p. 165.

The expression for k gives a critical value for the depth of the ocean, at which the thickness of the crust would seem to become infinite, and at the same time its density would become σ, the same as that of the substratum. This critical value is

$$\delta = \frac{\sigma - \rho}{\sigma - \mu} c.$$

We have assumed that $\sigma = 2{\cdot}96$ and $\rho = 2{\cdot}68$. The density of sea water is 1·028.[1] If then we proceed upon the assumptions we have just made, seeing that there are exceptional and limited areas, where the depth of the ocean attains five miles, we should be obliged to conclude that the thickness of the crust at the sea-board is at least 35 miles. But, setting aside these exceptionally deep areas, the average depth between 60° N. and 60° S. latitudes, where the ocean is on the whole deepest, may be taken at about 3 miles[2]; and it will be found that, if we assume $c = 25$ miles, which is the thickness that we have assigned to the crust at the sea-level, we obtain for δ, the depth of the ocean, the critical value 3·6 miles.

Taking the crust at the sea-board as 25 miles thick, we should have the following corresponding values.

Depth of the Ocean.	Thickness of Crust.	Density.
1 mile	32·15 miles	2·80
2 miles	49·34 miles	2·89
3 miles	124 90 miles	2·95
3·6 miles	∞	2·96

If we apply our results to places where the ocean is much over three miles deep, we obtain values for the total thickness of the crust of such a magnitude, as to make terms of the order $\frac{t^3}{a^2}$ no longer negligible; and consequently the approximation

[1] Herschel's "Physical Geography," p. 24.
[2] Wild's "Thalassa," p. 14.

appears to fail. It is however to be remarked that the places where the ocean is much over three miles deep occupy a comparatively small portion of the earth's surface[1], and it is therefore possible that our investigation might give us fairly reliable values for the thickness and density of the sub-oceanic crust at places at a distance from these deep spots under the conditions assumed; but it clearly fails for these and for places in their vicinity.

The explanation of the failure must be sought in the assumptions we have introduced to simplify the calculation. We have supposed the density of the crust, whether beneath the land or sea, to be uniform at any given place throughout its thickness. We have also supposed the density of the substratum not only to be uniform to the depth considered, but to be the same beneath the sea as under the land. This last named assumption would appear to contradict the results obtained in Chapter VI, where we gave reasons for believing that there must be convection currents in the substratum: and it is clear that, where the upward currents impinge on the crust, the density of the liquid would be less, perhaps on account of the greater vesicularity of the magma in those regions.

We have been restricted to our present degree of approximation by the fact, that the suppositions we have made furnish us with only two disposable quantities. If however we alter any of the three suppositions, we shall be enabled to introduce more conditions. Consequently we shall be able to satisfy equation (1) to a still higher degree of approximation, and at the same time to secure that the mass of a given frustrum shall be an absolute constant.

As a preliminary step in order to obtain general relations between the densities and thicknesses of any number of layers, thereby carrying the condition of uniformity of attraction to any desired degree of approximation, we may use the following method[2].

[1] Wild's "Thalassa," p. 14.

[2] This is an adaptation of a method used in Todhunter's "Theory of Equations," Art. 290.

Let there be n equations

$$x_1 + x_2 + x_3 + \ldots + x_n = b \quad \ldots\ldots(1),$$
$$a_1 x_1 + a_2 x_2 + a_3 x_3 + \ldots + a_n x_n = 0 \quad \ldots\ldots(2),$$
$$a_1^2 x_1 + a_2^2 x_2 + a_3^2 x_3 + \ldots + a_n^2 x_n = 0 \quad \ldots\ldots(3),$$
$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$a_1^{n-1} x_1 + a_2^{n-1} x_2 + a_3^{n-1} x_3 + \ldots + a_n^{n-1} x_n = 0 \quad \ldots\ldots(n).$$

Multiply equations $(n-1)$ by c_1, $(n-2)$ by c_2, $(n-3)$ by c_3, &c. &c. and (1) by c_{n-1}, and add the products to equation (n). Then, if we assume the values of the $n-1$ quantities, $c_1, c_2, c_3, \ldots c_{n-1}$, to be such that the coefficients of x_2, x_3, x_n in the sum vanish, we shall have

$$x_1 (a_1^{n-1} + c_1 a_1^{n-2} + c_2 a_1^{n-3} + \ldots + c_{n-2} a_1 + c_{n-1}) = c_{n-1} b.$$

The assumptions that we have made respecting the quantities $c_1, c_2, c_3, \ldots c_{n-1}$ amount to the same thing as supposing a_2, $a_3, \ldots a_n$ to be the roots of the equation

$$z^{n-1} + c_1 z^{n-2} + c_2 z^{n-3} + \ldots + c_{n-2} z + c_{n-1} = 0.$$

Wherefore

$$z^{n-1} + c_1 z^{n-2} + c_2 z^{n-3} + \ldots + c_{n-2} z + c_{n-1} = (z - a_2)(z - a_3) \ldots (z - a_n)$$

whatever be the value of z.

Also it follows that

$$c_{n-1} = (-1)^{n-1} a_2 a_3 \ldots a_n.$$

Thus we get

$$x_1 (a_1 - a_2)(a_1 - a_3) \ldots (a_1 - a_n) = (-1)^{n-1} a_2 a_3 \ldots a_n b;$$

or $$x_1 a_1 (a_1 - a_2)(a_1 - a_3) \ldots (a_1 - a_n) = (-1)^{n-1} a_1 a_2 a_3 \ldots a_n b.$$

So that if we have n quantities $x_1, x_2, x_3, \ldots x_n$, connected by $n-1$ equations of the form

$$a_1 x_1 + a_2 x_2 + a_3 x_3 + \ldots + a_n x_n = 0,$$
$$a_1^2 x_1 + a_2^2 x_2 + a_3^2 x_3 + \ldots + a_n^2 x_n = 0,$$
$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$a_1^{n-1} x_1 + a_2^{n-1} x_2 + a_3^{n-1} x_3 + \ldots + a_n^{n-1} x_n = 0;$$

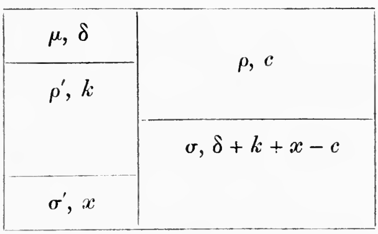
then we shall have the $n-1$ equations

$$\begin{aligned} x_1 a_1 (a_1 - a_2)(a_1 - a_3)\ldots(a_1 - a_n) & \\ &= x_2 a_2 (a_2 - a_1)(a_2 - a_3)\ldots(a_2 - a_n) \\ &= x_3 a_3 (a_3 - a_1)(a_3 - a_2)\ldots(a_3 - a_n) \\ &= \&c. \\ &= x_n a_n (a_n - a_1)(a_n - a_2)\ldots(a_n - a_{n-1}), \end{aligned}$$

which will give the values of the ratios $x_1 : x_2 : x_3 : \ldots : x_n$.

Let us now apply the formulæ just obtained to the crust of the earth upon certain hypotheses. We have already[1] considered the case of a continental crust of density ρ and a sub-oceanic crust of density ρ', both of them resting on a substratum of the same density σ; and need not repeat it.

Next suppose the density of the substratum under the ocean to be σ', differing from that beneath the land.



Substituting the proper quantities for the τ's and t's as was done in a previous case[2], this hypothesis will give us three equations, viz.

$$(\rho' - \mu)\,\delta + (\sigma' - \rho')(\delta + k) + (\sigma - \sigma')(\delta + k + x) + (\rho - \sigma)\,c = 0 \quad\ldots\ldots\ldots(4),$$

$$(\rho' - \mu)\,\delta^2 + (\sigma' - \rho')(\delta + k)^2 + (\sigma - \sigma')(\delta + k + x)^2 + (\rho - \sigma)\,c^2 = 0 \quad\ldots\ldots\ldots(5),$$

$$(\rho' - \mu)\,\delta^3 + (\sigma' - \rho')(\delta + k)^3 + (\sigma - \sigma')(\delta + k + x)^3 + (\rho - \sigma)\,c^3 = 0 \quad\ldots\ldots\ldots(6).$$

[1] p. 237. [2] See pp. 237, 238.

Applying the general formulæ lately obtained and changing the signs, these give

$$(\rho' - \mu)\,\delta k\,(k + x)\,(c - \delta) = (\sigma' - \rho')\,kx\,(\delta + k)\,(\delta + k - c) \ldots (7),$$
$$= -\,(\sigma - \sigma')\,x\,(k + x)\,(\delta + k + x)\,(\delta + k + x - c) \ldots (8),$$
$$= \quad (\sigma - \rho)\;\; c\,(c - \delta)\,(\delta + k - c)\;\;(\delta + k + x - c) \ldots (9).$$

The assumption of a density (σ') beneath the sub-oceanic crust different from that (σ) beneath the continental crust is in accordance with the belief in the existence of convection currents; and it is to be observed that we are thereby required to throw over the hypothesis of exact hydrostatic equilibrium, and can no longer regard the crust as floating upon a motionless liquid substratum. By assuming the density below the sub-oceanic crust to be σ', and that the density of the substratum becomes everywhere uniform at some unknown constant depth from the surface ($b = \delta + k + x$), we have two more disposable quantities, making four in all; and as we have only three equations to satisfy there is one quantity at our absolute disposal.

Eliminating ρ' between equations (4) and (5) we obtain

$$k = \frac{c^2 - 2\delta c + \dfrac{\sigma - \mu}{\sigma - \rho}\,\delta^2 - \dfrac{\sigma - \sigma'}{\sigma - \rho}\,x^2}{c - \dfrac{\sigma - \mu}{\sigma - \rho}\,\delta + \dfrac{\sigma - \sigma'}{\sigma - \rho}\,x}.$$

The critical value of the depth of the ocean which would make k infinite now becomes

$$\delta = \frac{\sigma - \rho}{\sigma - \mu}\,c + \frac{\sigma - \sigma'}{\sigma - \mu}\,x.$$

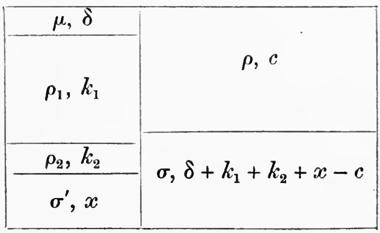
This will be greater than the critical value on page 239 if σ' is less than σ; and it is highly probable that we can make the critical depth to exceed five miles (which is the greatest recorded depth of the ocean) without assuming a larger value for $\delta + k + x$, or b, than is warranted by the order of approximation to $1/a^2$. This shows that, if the constitution of the crust which we are now treating is the true one, $\sigma - \sigma'$ must be positive.

But since every factor in the expression (8) is then positive,

this would require $\rho' - \mu$ to be negative, or the density of the sub-oceanic crust to be less than that of sea water; which is impossible. Hence our present assumption fails.

If we introduce two layers into the continental crust, so as to have six layers in all, three of different densities on each side, it comes out that the densities of the layers of the continental crust are functions of the depth of the ocean. This also is impossible, and we may dismiss the hypothesis.

Let us then try the hypothesis that the sub-oceanic crust is composed of two layers of thicknesses k_1, k_2, and of densities ρ_1, ρ_2.



We have in that case four equations, the first of which is

$$(\rho_1 - \mu)\,\delta + (\rho_2 - \rho_1)(\delta + k_1) + (\sigma' - \rho_2)(\delta + k_1 + k_2) + (\sigma - \sigma')(\delta + k_1 + k_2 + x) + (\rho - \sigma)\,c = 0,$$

and the rest as far as the fourth power of the thicknesses are in accordance with it.

The ratios of the density-differences will now be given by the four equations.

$$\begin{aligned}
&(\rho_1 - \mu)\,\delta k_1 (k_1 + k_2)(k_1 + k_2 + x)(c - \delta)\\
&= (\rho_2 - \rho_1)\,k_1 k_2 (\delta + k_1)(k_2 + x)(\delta + k_1 - c)\\
&= -(\sigma' - \rho_2)\,k_2 x (k_1 + k_2)(\delta + k_1 + k_2)(\delta + k_1 + k_2 - c)\\
&= (\sigma - \sigma')\,x (k_2 + x)(k_1 + k_2 + x)(\delta + k_1 + k_2 + x)(\delta + k_1 + k_2 + x - c)\\
&= (\sigma - \rho)\,c\,(c - \delta)(\delta + k_1 - c)(\delta + k_1 + k_2 - c)(\delta + k_1 + k_2 + x - c).
\end{aligned}$$

If in order to agree with the paragraph below all the factors expressing thicknesses are positive, it follows that all the density-differences are positive with the exception of $\sigma' - \rho_2$. There being six disposable quantities and four equations, two of the

quantities are arbitrary; and hence it appears that we may suppose the sub-oceanic crust to consist of two layers, the lower one being more dense than the substratum, the substratum itself being less dense beneath the ocean than beneath the land. We can make the two layers of the crust of any thicknesses we please within certain limits, giving corresponding values to x and the disposable densities. Although the lower layer (ρ_2, k_2) is more dense than the substratum under it, it is not necessary that taking its two layers into account, the mass of a column of the whole crust should be greater than what the mass of a column of the substratum substituted for it would be; so that the former need not sink into the latter.

It appears from the first of the above equations, that $\delta + k_1 - c$ must be positive if we are to have ρ_2 greater than ρ_1 and *a fortiori* $\delta + k_1 + k_2 - c$ must be positive. This shows that the sub-oceanic crust dips more deeply into the substratum than does the continental crust at the sea-board. But, if the conductivity of the sub-oceanic crust is not markedly less than that of the continental crust, considerations of temperature would lead us to think all the values given on page 240 are too large, and, where the depth approaches three miles, much too large. The ice-cold water found at the bottom of the deep oceans would only add about 1400 feet to the thickness of the crust as usually estimated. These reasons suggest that $\delta + k_1 + k_2 - c$, although positive, is small; and $k_1 + k_2$, that is the thickness of the solid crust beneath the ocean, may possibly be less than c, its thickness at the sea-board. Also since $\delta + k_1$ is greater than c, and $k_1 + k_2$ does not probably differ very much from c, it follows that k_2 is a quantity of the same order of magnitude as δ, and therefore admits of greater values where the ocean is deeper.

Now since ρ_2 is greater than ρ_1, this practically amounts to saying that the density of the sub-oceanic crust varies through its thickness, quite slowly in the upper parts, but very rapidly near the bottom.

The present hypothesis, which fulfils the condition of uniform attraction to the high order of approximation $1/a^3$, appears to be satisfactory. It gives the sign of $\sigma - \sigma'$ always positive, which

shows that there are upward currents everywhere beneath the ocean. This would lead us to expect that the thickness of the crust is certainly not much, if at all, greater there than beneath the land; the additional dip, or swag, of its underside being obtained solely out of the depth of the ocean; for the play of the currents against the lower surface would have a tendency to retard the thickening of the crust.

The circumstance that the currents ascend everywhere beneath the oceans, renders it necessary that the descending currents should have their position beneath the land; and that the former occupy so much larger an area is no more than we might expect; because to whatever immediate cause they may be due, they are ultimately the result of secular cooling. The cause producing them must therefore be everywhere present, and the descending being merely return currents will be confined to the smaller area[1]; but on that account they will move more rapidly.

We have seen that the density of the lower portion of the crust must in these regions be greater than that of the substratum because ρ_2 is greater than σ'. This appears highly probable; for if the defect of density causing the currents to ascend is due in any measure to vesicularity, knowing that the magma on slowly solidifying would extrude the gases, the solid additions accruing at the bottom of the crust must necessarily be more dense than the magma out of which they are congealed.

The mass of a frustum of the sub-oceanic crust must be greater than that of a similar one of the continental, because, while the mass of a frustum of depth b, in the two regions is the same, that of the ocean itself, and that of the substratum underneath it, being in defect, it follows that that of the sub-oceanic crust must be in excess. Thus if its thickness be equal to, or less than, or even not much greater than, that of the continental crust, its density must be greater.

We have stated that, although the density of the lower layer of the crust is greater than that of the substratum, it does not follow that the average density is greater, for, in places

[1] This behaviour of the currents is easily observed when a flocculent substance is mixed with hot water in an open vessel.

where the depth of the ocean does not exceed the mean, the denser layer k_2 will bear a comparatively small ratio to the whole thickness $k_1 + k_2$ of the crust. But in the few places where the depth approaches five miles, it is possible that the denser layer may be so thick as to render the average density of the whole somewhat greater than that of the substratum. This might be due to the currents at that spot being less powerful, and consequently the freezing at those places might be more rapid. In this case the crust might be slowly sinking. "Several soundings exceeding 4000 fathoms were obtained by the Tuscarora to the eastward of the Islands Niphon and Yezo, and another close to the most westerly of the Aleutian Islands[1]." Now it is known that most of the earthquakes which disturb the much shaken islands of Japan originate beneath the sea on that side; which shows that the sub-oceanic crust in that region is in a very unstable condition, as it would be if it were thus sinking.

Since upward convection ceases at the sea-board, there must be some depth of the ocean which corresponds to a maximum play of rising currents. This may probably be indicated by areas that from the great upward pressure may be slowly rising, so as to form the remarkable plateaux which occupy extensive tracts of the ocean bottom. It is on these plateaux that the volcanic islands of mid-ocean are based, and it is obvious that the condition of the molten substratum with upward currents pressing against the under side of the crust is exactly that, which would tend to rupture it, and open fissures, and originate volcanic vents.

The conclusion at which we have arrived regarding the relative densities and thicknesses of the crust, sufficiently accounts for the collection of water in the oceanic areas, because it has been obtained subject to the consideration that the water is there. It appears therefore that this collection of water may be attributed to increased local density of the crust, and does not require increased local density in the more deeply seated matter.

It is obvious that our results are strongly confirmatory of the

[1] "Thalassa," p. 15.

theory of the permanence of ocean basins, because it is difficult to conceive how the subjacent crust, once more dense, can have subsequently passed into the less dense condition which would be requisite to render it continental.

It will be recollected that, in estimating the thickness of the crust and the melting temperature by means of comparing the rates of underground temperature in mountains with the average rate, we obtained smaller values than expected[1]. This comparison was made upon the hypothesis of exact hydrostatic equilibrium; and it appeared from our equation that, if the densities of the crust and substratum were more nearly equal than had been assumed, the values of these quantities would have come out larger. We have now arrived at the conclusion that in all probability there are descending currents in the substratum beneath mountainous areas and these would have a hydrostatical effect similar to what a closer approximation of the densities would have, because some part of the support, which the roots of the mountains would experience if the liquid was motionless, would in that case be withdrawn. Again, when we were discussing the connection of gravitational phenomena with the roots of mountains[2] we stated that it was possible, that the convection currents, of whose existence we were already assured[3], descended beneath mountainous regions, and that this might be a reason why the roots were not being rapidly melted off, although projecting abnormally deeply into the substratum. The results of the present chapter in determining the relative positions of the ascending and descending currents go some way towards confirming this view.

[1] p. 229. [2] p. 203. [3] p. 77.

CHAPTER XVIII.

ISLAND ATTRACTION.

Gravity at island stations usually in excess—Pratt's explanation of this—M. Faye's is similar—Falkland Islands, an exception—Geological significance of the phenomena—Distinction between volcanic and non-volcanic islands—Gravitational effect of the cone of Fujisan—Table of variations of gravity at seventeen island stations.

It has long been known that gravity at island stations in the open ocean is in excess of its normal value. In the preceding chapter it has been shown that, if the earth was spherical, the value of gravity at the sea level would be everywhere the same. And when we take account of the ellipticity which is due to rotation, that constant value of gravity will need to have applied to it simply the correction for latitude according to Clairaut's law in order to give the true value at any place. And there can be little doubt that, if it were possible to make pendulum observations on board ship, such would be found to be the case. But since it is not practicable to do this, pendulum observations for oceanic stations have necessarily been made upon islands; and it has almost invariably been found, that gravity at island stations is greater than had been expected. Archdeacon Pratt explained how this excess is necessarily produced by the attraction of the rock of which the island itself is composed[1], for if the

[1] "Figure of the Earth," 4th ed., Artt. 75, 196. 1871.

island were not present, its place would be occupied by water, and gravity would have then the exact value for the latitude. But the excess of density of the rock over that of water, and its proximity to the pendulum, sensibly increases gravity upon the surface of the island.

M. Faye has treated of this question[1]. His conclusions are almost identical with those of Pratt; to whose work however he does not allude. He shows that, if the density of a circular conical island be taken at 2·5, its height above the sea bottom at 4500 m. (2·796 miles), and the radius of its base from one to two times its height, then the number of vibrations made by the seconds' pendulum would be increased by from 3 to 6 vibrations *per diem*. Now the mean increase at island stations is 5·26 vibrations. Thus the excess of gravity at such stations is satisfactorily accounted for.

There is however a remarkable exception to the general rule in the case of the Falkland Islands. There the variation of gravity is in defect, and the defect amounts to 3·85 vibrations *per diem*.

Let us consider the geological significance of these phenomena. It has already been proved[2] that a downward protuberance, or "root," beneath a mountain range, consisting of rock of surface density projecting into the denser substratum, will account for the fact that the mass of the mountain has scarcely any effect upon gravity at its summit, seeing that the root almost counteracts the attraction of the mountain. Now an island is a mountain standing on the sea bottom. If then an island were to have a root of its own diameter, and of less density than the substratum, the increase of gravity at its surface ought not to be as great as the mere substitution of rock for water would make it. But in fact the substitution of rock for water brings the calculated increase very near the actual increase. From this the conclusion follows, that the island has no root of its own diameter. In the case of a volcanic island there are two causes, either of which would prevent the counterbalancing effect of the root being produced.

[1] "Comptes Rendus," 22 Mar. 1886.

[2] p. 212.

In the first place the sub-oceanic crust is dense, and even if there were a root it would differ but little in density from the substratum. But in the second place a volcanic mountain has not been elevated by compression, but is a mere heap of ejected materials. Consequently no downward protrusion accompanies its formation, and if there be anything of the nature of a root, it will probably be simply a wide depression, or sagging, of the crust, caused by the load which is laid upon it. This would affect the pendulum only slightly, being wide rather than deep, and the vertical component attraction of its circumferential parts would be scarcely felt on the island, on account of their being too far away.

On the other hand, the Falkland Islands are not volcanic; and may be regarded as an outlying fragment of the South American continent. How they came to exist where they are, involves an interesting question. They consist of disturbed strata of silurian age, containing fossils, are much contorted, and have doubtless roots projecting into the denser substratum, and as a consequence the variation of gravity there is, contrary to the usual rule for islands, found to be in defect[1].

A very remarkable instance which illustrates the gravitational effect of a volcanic cone is recorded in the case of the lofty Fujisan in Japan. This cone is 12,000 feet high and tradition says it was thrown up in a single night, about 300 B.C. The mountain attracts the pendulum exactly as a mass of the same size would do if it had been carted thither and piled up[2]. This not only shows that it has either no root, or a mere wide depression of the crust, but it also proves the important fact that the enormous amount of material ejected from the vent, and of which the cone itself is probably only a part, has left no appreciable void beneath. It has in short

[1] See Darwin's "Naturalist's Voyage," 2nd ed., Chap. IX., p. 196. 1845. Spitzbergen ought perhaps not to be included in this category on account of its large size. It appears to consist of tertiary strata in most parts horizontal. Nordenskiöld, "Nature," vol. 39, p. 491.

[2] "United States Coast and Geodetic Survey." Appendix 22, p. 507. Washington, 1883.

been derived from a wide spread contribution out of the immense reservoir of the general substratum of molten magma.

The following table of variations of gravity at island stations is copied from M. Faye's article.

Station	Latitude	Observed Variation	Observers
Spitzbergen	80° N.	+ 3·09	Sabine.
Portobello	10° N.	+ 3·85	Foster.
Galapagos Is.	6° S.	+ 2·43	B. Hall.
S. Thomas	0°	+ 6·86	Sabine.
P. Gausea Lout	0°	+ 4·53	Goldingham.
Fernando de N.	4° S.	+ 8·22	Foster.
Ascension	8° S.	+ 6·15	S. and F.
S. Helena	26° S.	+ 9·32	Foster.
Staten I.	55° S.	+ 2·90	Foster.
S. Shetland	63° S.	+ 3·90	Foster.
Minicoy	8° N.	+ 3·49	English Officers.
Mowi	21° N.	+ 4·80	De Freycinet.
Ile de France	20° S.	+ 7·16	De Freycinet.
Iles Malouines (Falkland Is.)	52° S.	− 3·85	De Fr. and Duperré.
Ile Bonin	27° N.	+11·79	Lutke.
Guam	13° N.	+ 4·88	Lutke.
Occalon	5° N.	+ 9·93	Lutke.
		Mean + 5·26	

CHAPTER XIX.

AMOUNT OF COMPRESSION.

Compression possibly confined to continental areas—Compression might arise from extravasation of matter from beneath the crust, or from expansion of the crust—Amount of compression needs to be estimated afresh for mean level—Datum level equation transformed to mean level—Formula to express compression in terms of inequalities—Estimate of inequalities of land surface—The corresponding compression—Sources of error—Vertical uplifts—Position of mean level uncertain—Extravasation of water will not account for compression.

In the seventeenth chapter we made it apparent that the depressions occupied by the great oceans are probably the result of greater density of the crust, and that it is probably not much thicker beneath the oceans than beneath the continents, even at the sea-board. This leads to the conclusion that the compression, which has caused the thickening accompanied by corrugation, such as characterises most elevated tracts, is properly a continental phenomenon, and has no analogue beneath the oceans. If therefore we desire to estimate the amount of compression, we shall be probably justified in confining our attention to the continental areas.

There are two ways in which compression may have acted to elevate any tract of the crust, supposed to rest on a liquid substratum. In the one the anticlinals would form ridges, whose sections would be cusp-like and the subjacent fluid would rise into them. This hypothesis has been discussed in the

twelfth chapter, and has been shown not to accord with natural appearances. It is one however which has been more or less assumed in many geological writings[1]. The other is that developed in the thirteenth chapter, and shown in the fourteenth, fifteenth, and sixteenth, to be capable of explaining two classes of phenomena perfectly independent of each other, but which, it may be observed, would be absolutely reversed were the doctrine true, that the heavier and hot molten liquid rises into the anticlinals: because in that case the attraction of mountain masses would be greater than if they consisted of matter of the mean density of the crust, instead of being, as it is, less: and the rate of increase of temperature within mountains would also be greater on account of the proximity of the molten matter, instead of being, as it is, less.

We have then to seek for the cause of this compression, which has affected the continental areas.

If we have given a sphere of a certain radius, whose outer crust is solid and rests upon a liquid substratum, compression of the crust may arise from contraction of the volume of the sphere, either through cooling, or by the extravasation of some portion of the matter which was originally beneath the crust. In these cases the compression of the upper surface of the crust would be equal to the contraction of the sphere; that is to say, if the radius of the sphere before contraction was $r + cr$, and after contraction became r, c being the coefficient of contraction, then the mean coefficient of compression along any line drawn upon the sphere would also be c. If the length of the line after compression was l, then before compression it must have been $l + cl$.

But there may be another cause for the compression which has elevated continental areas. The solid crust may have increased horizontally. It is quite possible that these two causes of compression may have coexisted. The sphere may have contracted, and the crust grown larger simultaneously; in which case the contraction of the volume of the sphere will be an inadequate measure of the compression of the crust.

We have already made it apparent, that the cooling of the

[1] See Prof. A. de Lapparent, "L'Origine des inégalités de la Surface du Globe." "Revue des Questions Scientifiques." Juillet, 1880.

earth considered as a solid sphere cannot account for the inequalities, and therefore neither sufficiently for the compression of the crust. And if we regard the crust as resting on a liquid substratum, it does not appear how the contraction through mere cooling can in that case have been so much greater than in the case of a solid globe, as to account for what the latter will not. We have therefore to examine whether other hypotheses will more satisfactorily explain the phenomena. But it is obvious that a preliminary step, before we can reason at all about the compression under the condition of a fluid substratum, must be to estimate its amount afresh: for the grounds upon which that must be done are very different from those upon which we formerly estimated the compression in the case of a solid globe[1].

If we revert to the datum-level equation (1) in the seventh chapter[2], which is perfectly general for a vertical section of the crust under compression, and adapt it to volumes, as has been done in equation (2)[3], but retaining terms answering for volumes to $\Sigma(\alpha)$ and $\Sigma(\beta)$ in equation (1), which terms we will call $\Sigma(X)$ and $\Sigma(Y)$, we then have for any area L,

$$2kLc = \Sigma(A) - \Sigma(B) + \Sigma(Y) - \Sigma(X),$$

for our general datum-level equation of volumes for a solid crust resting on a liquid substratum, c being the coefficient of compression. And we know that in this expression

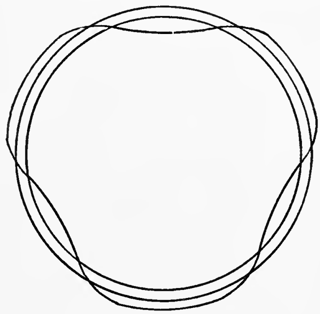
$$\Sigma(Y) - \Sigma(X) = 0.$$

In this equation it will be recollected that c is the mean linear compression of the crust, and that the surface above which the volumes $\Sigma(A)$, and below which the volumes $\Sigma(B)$, are reckoned, is the imaginary surface which occupies the position that the upper surface of the crust would occupy at the present time, had it been perfectly compressible in a horizontal direction; and that $\Sigma(X)$ and $\Sigma(Y)$ are the volumes similarly situated with respect to the lower surface of such a crust[4]. Let us now transform this equation to the upper and lower "mean

[1] See Chap. IX., p. 112.

[2] p. 85. [3] p. 87. [4] p. 87.

levels" of the crust, which by the definition will be at the same distance apart as the datum levels[1].



In the same manner that A, B, X, Y are referred to the datum levels, let A', B', X', Y' be referred to the mean levels; and, in the figure, let in the first place the outer circle be the lower mean level, and the inner one the lower datum level; and let the distance between the two circles, that is the distance between the lower mean and the lower datum level, be ϵr, where r is the radius of the sphere to the upper datum level. Then since k is the mean thickness of the crust,

$$4\pi\epsilon r\,(r-k)^2$$

is very nearly the volume of the shell between these two levels. And so it is easily seen from the figure, that

$$4\pi\epsilon r\,(r-k)^2 = \Sigma\,(X) - \Sigma\,(X') + \Sigma\,(Y') - \Sigma\,(Y).$$

But we know that, by the datum-level equation, since[2] $\Sigma\,(\alpha) = \Sigma\,(\beta)$, that

$$\Sigma\,(X) - \Sigma\,(Y) = 0,$$

$$\therefore\ 4\pi\epsilon r\,(r-k)^2 = \Sigma\,(Y') - \Sigma\,(X'),$$

and

$$\epsilon = \frac{\Sigma\,(Y') - \Sigma\,(X')}{4\pi r\,(r-k)^2}.$$

Secondly, suppose the outer circle to be the upper mean level, and the inner one the upper datum level. Then the volume of the shell between these two levels is nearly $4\pi r^2\epsilon r$.

[1] p. 166. [2] p. 86.

And from the figure,

$$\Sigma (A) - \Sigma (A') + \Sigma (B') - \Sigma (B) = 4\pi r^2 \epsilon r;$$

$$\therefore \Sigma (A) - \Sigma (B) = 4\pi r^2 \epsilon r + \Sigma (A') - \Sigma (B') \ldots\ldots (\text{A}).$$

But, by the datum-level equation,

$$\Sigma (A) - \Sigma (B) = 2kLc.$$

And the value of ϵ has just now been found.

Hence, substituting for $\Sigma (A) - \Sigma (B)$ and ϵ in (A),

$$2kLc = \frac{r^2}{(r-k)^2} \{\Sigma (Y') - \Sigma (X')\} + \Sigma (A') - \Sigma (B') \ldots\ldots (\text{B}).$$

It has also been shown that, in the case of a section of unit width, if $l\delta$ be the volume of water that may have covered the tract before there were any elevations, and $\Sigma (d)$ the volume of the water which rests upon it after elevation, for a section across a disturbed tract of unit of width we have the two relations[1],

$$\Sigma (a) - \Sigma (b) = \frac{\sigma - \rho}{\sigma} klc + \frac{\mu}{\sigma} \{\delta l - \Sigma (d)\} \ldots\ldots (1),$$

and

$$\Sigma (\beta) - \Sigma (\alpha) = \frac{\rho}{\sigma} klc - \frac{\mu}{\sigma} \{\delta l - \Sigma (d)\} \ldots\ldots\ldots\ldots (2).$$

If instead of a section of unit of width we substitute an area L, these equations become

$$\Sigma (A') - \Sigma (B') = \frac{\sigma - \rho}{\sigma} 2kLc + \frac{\mu}{\sigma} \{L\delta - \Sigma (D)\},$$

and

$$\Sigma (Y') - \Sigma (X') = \frac{\rho}{\sigma} 2kLc - \frac{\mu}{\sigma} \{L\delta - \Sigma (D)\}.$$

As has been shown for $\delta l - \Sigma (d)$ in the case of unit of width[2], so it is evident here that $L\delta - \Sigma (D)$ is the water displaced by rock owing to the elevation, and $\Sigma (D)$ the volume of any water which may happen to lie over it.

[1] p. 167. [2] Ibid.

Transposing, we have by division the proportion

$$\frac{\Sigma(A') - \Sigma(B') - \frac{\mu}{\sigma}\{L\delta - \Sigma(D)\}}{\Sigma(Y') - \Sigma(X') + \frac{\mu}{\sigma}\{L\delta - \Sigma(D)\}} = \frac{\sigma - \rho}{\rho} \quad \ldots\ldots(3).$$

In our ignorance of the mode of evolution of the continents, we cannot tell whether geological history goes back to a time when the globe was covered by a universal ocean; or whether on the other hand there have been continental areas ever since the waters were first condensed. Neither can we say how much water has been displaced by the direct protrusion of rock within the areas now occupied by land, that is how much, if at all, the continents have increased in size; nor on the other hand do we know how much land has been carried by denudation into the sea; nor how much has sunk by direct depression beneath the water. This much however we are able to do. We can estimate how much compression is implied in the supposition, that the surface of the land, as we now see it, was once all at the sea level, and that it has been elevated by lateral compression alone to its present height.

In this case we may put δ and $\Sigma(D)$ each equal to zero, and we then obtain

$$\Sigma(Y') - \Sigma(X') = \frac{\rho}{\sigma - \rho}\{\Sigma(A') - \Sigma(B')\}.$$

And substituting in (B),

$$2kLc = \frac{r^2}{(r-k)^2}\left[\frac{\rho}{\sigma-\rho}\{\Sigma(A') - \Sigma(B')\}\right] + \Sigma(A') - \Sigma(B');$$

whence

$$c = \left\{\frac{\rho}{\sigma-\rho}\,\frac{r^2}{(r-k)^2} + 1\right\}\frac{\Sigma(A') - \Sigma(B')}{2Lk}.$$

If we can estimate $\Sigma(A') - \Sigma(B')$, we can now find c the coefficient of the compression, which would raise the surface to its present mean height above the sea level.

$\Sigma(B')$ which is the volume of depressions below the mean level will be zero. If we then adopt Professor Haughton's

estimate of the mean height of the land, viz. 1000 feet[1], or 0·19 mile, we have,

$$\Sigma(A') - \Sigma(B') = 0{\cdot}19L,$$

$$r = 3959,$$

$$k = 25,$$

$$\frac{\rho}{\sigma - \rho} = 9{\cdot}57.$$

This gives $c = 0{\cdot}04$, for the coefficient of compression.

The continents would therefore need to have undergone a mean linear compression of about 4 *per cent.* to raise them from the sea level to their present mean height.

This estimate is subject to many sources of error. In the first place it is by no means certain that the elevation of all land areas has been caused by lateral compression, although compression has undoubtedly played an important part in most cases. The recent investigations of the Geological Survey of the United States have demonstrated, that the lofty Plateaux, which constitute the water-shed of North America, have been uplifted by a subterranean force acting vertically. The area of this district amounts to about 130,000 square miles, and its mean altitude is about 6,500 feet[2]. Still, immense as it is, it forms but a small part of the entire continent, and, as far as the geological investigation of all other countries has been carried, it appears to be unique in its features.

Another point on which we have no reliable data is, as to how far we are justified in regarding the surface of the sea as the bench mark, above which compression has elevated the land. It may be, and probably is, the case that we ought to go deeper than that, because the sea bottom does not assume that level character which appears to belong to the deep oceans, until some distance from the shore line is reached[3]. But on the whole

[1] p. 112.

[2] "Sixth Annual Report of the United States Geological Survey," p. 117.

[3] In the case of the Pacific this is stated to be 40 or 50 miles. "Californian Academy of Sciences Bulletin," vol. II., no. 6. See "Nature," vol. XXXVII., p. 38, 1887. See also some interesting remarks by Mr John Murray and Rev. A. Renard in a paper before Roy. Soc. Edin. "On the Nomenclature, Origin, and

it seems probable, that a mean compression of 4 *per cent.*, which will have reduced 104 miles run to 100, would according to our view of the constitution of the crust, in which we assume the relative densities of the crust and substratum to be 2·68 and 2·96 respectively, and the thickness of the crust at the sea-board to be 25 miles, have elevated the continents from the sea level to their present height.

A suggestion formerly made by the writer, that the extravasation of water from beneath the crust by means of volcanic eruptions may have been an appreciable cause of compression, has been already referred to as having had to be abandoned[1]. It is certain that this water must have gone to increase the volume of the ocean, and its emission from beneath the crust must have caused a direct diminution of the volume of the interior. This then falls under the case, where the coefficient of compression will be the same as that of contraction. We have no means of knowing how much of the volume of a given mass of the magma may be due to the water, which is held in solution. Probably the increase from this cause is small. If however it were increased by the entire volume which the water possesses in its liquid state, the volume of the nucleus before the extravasation could only have been formerly larger by the volume of the ocean. Putting the area of this at 145 millions of square miles, and that of the whole globe at 197 millions, and the mean depth of the ocean at 3·5 miles, the depth of this when spread over the surface of the sphere would have been $3{\cdot}5 \times 145/197$ miles; and the radius is 3959 miles. Hence the coefficient of contraction, and therefore of compression arising from this cause would at the utmost be only

$$3{\cdot}5 \times 145/197 \times 3959 = 0{\cdot}0006.$$

This cause of compression could not have produced any appreciable results.

Distribution of Deep-Sea Deposits," reported in "Nature," vol. xxx., pp. 84, 114, 132, especially the latter part p. 134, where two-eighths of the earth's surface is estimated as being the portion "in which all or nearly all the sedimentary rocks of the continents have been built up."

[1] p. 139.

CHAPTER XX.

DISTURBANCE OF ROCKS.

Disturbance of rocks consists of—(1) Vertical changes of level which have affected the ocean bottom as well as the continents—(2) Lateral compression producing large folds, the general type of mountain ranges—(3) Shearing—Why shearing stress will sometimes be satisfied by faulting and sometimes by quasi *viscous shearing—Connection between a viscous shear and crumpling—How a crumpled rock passes into a schist—Distinction between a schist and a slate—Distortion of fossils—Great amount of shear observed in some districts —(4) Faulting—Course of faults in straight lines—Fissures occasioning faults may commence at the surface—Faults in Utah—Faults superimposed on corrugations—Extent of throw of great faults—These may give rise to fissure eruptions.*

THE great problem of physical geology is to account for the disturbances which have affected the crust of the earth. The chief of these may be classed under the four headings of (1) vertical changes of level; (2) lateral compression accompanied by folding on a large scale, such as has elevated mountain chains; (3) shearing of the beds over one another, accompanied by crumpling on every scale of magnitude; (4) faulting. The task which we have undertaken consists rather in an attempt to explain the manner in which these disturbances have been caused than minutely to describe them. They are the subjects to which the attention of field geologists has of late been expressly directed, and the results of their observations have been abundantly recorded. It will be sufficient here to refer very succinctly to the nature of the phenomena.

(1) Changes of level seem on the whole to be the most universal of all kinds of disturbance. They are even now going on, as is evidenced by the encroachment of the sea upon some coasts and its retreat from others.

There are clear proofs that the crust beneath the ocean has been subject to changes of level[1], although there are no indications of compression, except in the case of a very few islands which are not situated in mid ocean[2]. There have also been lately explored in the United States elevated plateaux, which have not been subjected to compression ever since they were beneath the sea, but appear to have been raised bodily by some force acting vertically from below.

(2) In the more simple type of mountain ranges the strata have been thrown into great folds. The Jura mountains are an instance of a simple structure of this nature. The Appalachians are somewhat more complicated. The loftier ranges of the globe, the Alpine and Himalayan system, and the Andes, show the same fundamental type of structure, though much complicated by superimposed movements.

In these instances, from whatever cause arising, the lateral pressure appears to have acted mostly in a direct manner, inducing compression and a consequent thickening, or bulging, of the crust of the earth in some such manner as we have indicated in Chapter XIII. Often this kind of direct compression has been on a grand scale. In the Appalachians it has been estimated by Prof. Claypole that 100 miles run of the surface has been compressed into about 65 miles, which he considers an under estimate[3]. This gives the present as about 2/3ds of the original dimension, or the compression amounts to one third. Prof. A. Favre of Geneva gives the same amount of compression, viz. one third, as having been exerted on certain mountains of Savoy[4].

(3) We will consider somewhat more in detail the character

[1] "Guppy's Solomon Islands," p. 126. London, 1887.

[2] E.g. South Georgia, The Falkland Is., &c.

[3] "British Assoc., Montreal," 1884. See "Geol. Mag.," Dec. III., vol. I., p. 466, 1884. The paper appears in full in "The American Naturalist," March, 1885.

[4] p. 179.

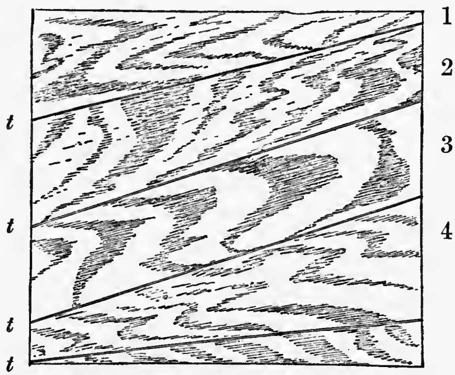
of the phenomena, which fall under our third heading, and which have been heretofore commonly referred to "earth-pressure," "crust-creep," etc.; the distinction between pressure and shearing stress having been imperfectly appreciated. At the same time no very serious attempts have been made to discover the cause of these movements. Geologists however have lately recognised the fact, that crumpling of rocks passes by gradations into mere schistosity, and that schistosity is the consequence of a shearing movement within the mass of the rock, as was long ago pointed out by Herschel[1].

As a matter of fact, every "fault" indicates a shear. But the kind of shear now referred to is one, in which the "throw," that in faulting occurs along a single shearing plane, the "fault-," or "thrust-plane," is in this case distributed as a viscous shear throughout a considerable interval in the rocks. In the case of a fault, one may stand with the two feet planted on rocks, whose mutual displacement perhaps amounts to hundreds of yards. But in the case of a shear of the kind now referred to, one might be obliged to cross some miles of country to reach a similar amount of displacement. A viscous shear of this kind may be defined in its mode of production as a movement, by which the material, on the opposite sides of a selected surface to which we refer it, are constrained to move with different velocities; the velocity, as referred to the selected surface, increasing more and more as we recede to a greater distance away from it. Whether a shearing stress shall be satisfied by a slip along a particular plane (which would be a "fault-plane" or "thrust-plane"), or whether it shall be satisfied by a viscous shear such as described, will depend upon the ratio of the shearing stress to the friction. If the shearing stress exceeds the friction, a fault will be formed; but if it be less, there will be only a viscous shear. Now pressure normal to the direction of the shearing stress will have but little effect on the viscosity, but it will have a very great effect upon the friction. From this cause alternations of periods of viscous shearing and faulting might occur again and again according as

[1] "Phil. Mag.," vol. XII., p. 198. See also Phillips, "On certain movements in the parts of stratified rocks." "Brit. Assoc.," 1843.

the normal pressure was increased or diminished; and these conditions might be brought on by the shearing action itself thrusting in wedges of rock, and so increasing the pressure, or thrusting them out and reducing it. From these considerations it seems probable that instances might occur, in which a long continued shearing stress might be continuously satisfied by a slow viscous shearing, and that the stress might on that account never have the opportunity of increasing to a sufficient amount to overcome the adhesion and friction, and so to induce faulting. It is however a common occurrence, to find the visible records of both viscous shearing and faulting occurring together. This combination of the two is well shown in the accompanying diagram, in the description of which the nearly horizontal reversed faults are termed "thrust-planes." The figure is copied, by permission of the Council of the Geological Society, and of Dr Geikie, Director General of the Geological Survey, from a "Report of the work of the Survey in the Highlands[1]."

"*Section of original Archæan Gneiss, showing thrust-planes, oblique foliation and overfolding.*"



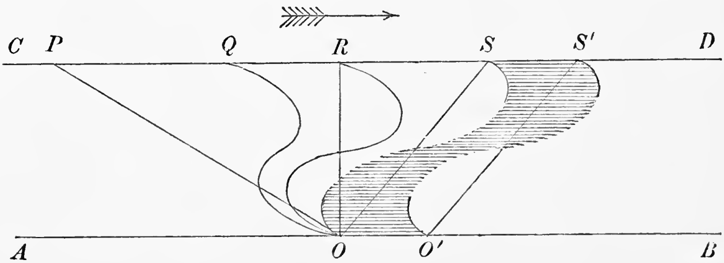
"*t*, *t* thrusts."

"The figure represents an area of several hundred square yards, the plane being vertical, and the observer facing the South."

It will appear from what follows, that in the above instance

[1] "Quart. Journ. Geol. Soc.," vol. XLIV., p. 389, 1888.

the rock situated between the "thrust-planes" shows the effects of viscous shearing.



The above diagram is hypothetical, and represents a section of a portion of rock, including layers harder than the general matrix, as it would appear at successive intervals while undergoing a shear. *OP* is a section of the surface of one of the hard layers before the movement commenced. The curved line *OQ*, which is drawn of the same length as *OP*, is the same surface, while the layer is undergoing compression. *OR* is the same at the time of maximum compression. *OS* is the same later on, after stretching has begun in the middle parts of the fold. The positions of the particles, along this surface *OS* in the two middle quarters, are determined by setting off points on either side of the axis *OS*, at the same distances measured along the direction of shear, as the particles occupied with respect to the axis *OR*, when it was in the middle position. But, towards the extreme parts of the fold, the particles will be further from the axis than they were from *OR* as explained in the text. *O′S′* is the other surface of the hard layer corresponding as to time and position with *OS*.

The connection between a viscous shear and crumpled laminæ, or "over-folding," may be thus explained[1]. Suppose the rock between the planes *AB* and *CD* to be subjected to a shearing stress, tending to cause a movement of the material above *AB* from left to right. We may refer it to the plane *AB*, as if *AB* was stationary, although in fact the particles along *AB* are themselves moving from left to right with reference to those below them. Before the movement commenced, suppose the rock to have consisted of parallel layers of unequal hardness, inclined towards the direction of shear, and let *OP* be a section of a surface of one of the harder layers. It is clear that, as the movement goes on, the particles at *O*

[1] See "Wie am Mte. Piottino die Parallelstruktur des Gneisses in Schichtung übergeht. Von Dr F. M. Stapff in Airolo. Neuen Jarbuch für Mineralogie Geologie und Palæontologie. Jargang, 1882. 1-Band."

and P will be forced nearer together, and the layer between them will be crumpled up. Every similar layer will be similarly affected, and while their compression is going on the velocity will exceed the mean in the front folds, which face towards the direction of shear, and it will be less than the mean in the back folds, which look the opposite way. The mean velocity will be maintained at the extremities and at the middle point of the fold, being represented for every layer by the velocity of the corresponding point of the axis of the fold.

It seems pretty certain that the number of folds thus produced would be more numerous, when in a given thickness of the rock affected the harder layers were numerous and thin, than when they were thick and far between. For as already noticed, while the folds are being formed, what we may call their front members are moving faster, and the back members more slowly, than with the mean velocity of the shear. Now it is evident that, if the layers are thin and numerous, the rock approximates to homogeneity; in which case there would be no perceptible inequality of shear, and no folds formed at all: that is to say they would be infinitely small, and infinitely numerous. We may therefore conclude that the thinner and more numerous the alternations of hard and soft layers are, the more numerous will be the folds; and *vice versâ*. The character of the folds in the gneiss at Oriboll[1], as shown in the diagram, confirm this conclusion. But the irregularity of the foldings within very short spaces, points to a *quasi* liquidity, which is very remarkable; and indicates the presence of a long continued stress acting very slowly. The different directions of the axes of the folds in the second and fourth natural divisions may perhaps be accounted for by the folds in the second representing the layers before, and in the fourth after, the axes had reached the position, where they would have been orthogonal to the shear: or possibly the shear in the second division has been from left to right in an opposite direction from that in the fourth which has been from right to left.

For simplicity confining our attention to a simple sigmoid fold, when the movement has brought P to R, opposite to O,

[1] p. 264.

the layer under consideration will have undergone its maximum of crumpling; every part of it having been subjected to compression all the while. After having passed that middle position, compression will give place to extension in the two middle quarters of the fold; and the rigidity of the fold will there lose most of its effect in interfering with the velocity of the shear. In this part of the fold therefore the particles would subsequently travel with the mean velocity, and be always arranged at nearly the same distances on either side of the axis of the fold in each new position, as they occupied when it was in its middle position. Some amount of compression would continue to affect the extreme parts of the fold, these being separated from the extended parts at the points where the fold is perpendicular to the direction of the shear. In the former the front portions would continue to be somewhat accelerated, and the back ones to be retarded, so that the contour of the elbows so formed would gradually grow less rounded, or more zigzag, and at the same time, measuring across the shear, they would recede from one another. In this manner the shearing process would eventually straighten out the folds. But it will be observed that, in this straightening process, the limiting positions of the two elbows of the fold being necessarily on its axis, they will never recede past the parallels *CD* and *AB*. The zigzag form, which the folds assume during this process of opening out, is well illustrated in the second and fourth natural divisions in the diagram of the gneiss.

When the straightening out was completed the layers would have become flat, and much reduced in thickness; so that measuring across them there would be found more numerous alternations of hard and soft, thinner than the original layers. It must be observed however, that if the shearing movement is subsequently continued, it will not be along the now flattened out layers, for they will always have been somewhat inclined to the direction of the shear.

By the process described, a schist would finally be elaborated out of what was originally a laminated rock, and in an earlier stage of shearing a crumpled rock. A schist thus formed,

might at some subsequent period be subjected to a differently directed shear, and the same process being enacted over again upon its layers, a later formed crumpled rock, passing into a newer schist, might be formed out of the old one.

By shading the space between *OS* and *O′S′*, the two surfaces of the hard layer at a somewhat advanced stage of the movement, we see what appearance a fold crumpled according to our theory would present; and it is remarkable how closely it resembles the more simple form of the crumpled mass of gneiss, in the third natural division in the diagram: although the sigmoid folds therein are not entire, but have been subsequently truncated by the bounding thrust-planes.

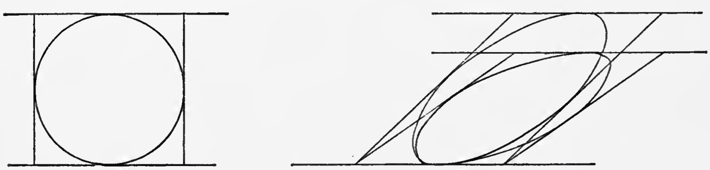
The closeness of the folds of a crumpled rock, formed as supposed, would depend upon the angle at which the direction of shear met the layers of the rock, when the shearing and consequent folding first began. The smaller this angle, the closer would be the folding. If on the other hand the angle in question was a right angle, or anything greater than a right angle, there would be no folding. In that case the particles of the layers would be at once sheared over each other, and simple schistosity produced. Thus it appears that the same amount of shear, which at one locality produces crumpling, may in another not far off produce schistosity: the difference in effect arising from original different inclinations between the shear and the layers affected at the two localities.

In schists proper the planes of schistosity, along which the rock cleaves most readily, no doubt represent the planes of shearing. This arises, as Herschel remarked, from "the tendency of the particles to arrange themselves when in motion all in one direction, according to the laws, *not of pressure* but *of friction*, a distinction which is quite necessary to be borne in mind[1]." But besides the particles being thus turned round, and arranged with their lengths in one direction, they undergo abrasion, and probably also chemical solution, so as to be reduced to the eye-shaped form so general in schistose rocks; and their flat sides then guide the cleavage.

[1] "Phil. Mag.," vol. XII., p. 198.

It appears however that the cleavage of a true slate must be caused in a somewhat different manner, the proof being that, where the cleavage of a slate happens to coincide with the original bedding of the deposit, if there are any fossils, they are found to be lengthened in the direction of the cleavage. This shows that the plane of slaty cleavage is a plane of distortion, which the plane of shear is not.

That the plane of shear is not a plane of distortion may be thus shown. The diagram illustrates the effect of a shear in



altering the forms of a square, and of a circle, both with and without compression. We may regard the square as the section of a cube, and the circle as that of a sphere, and we see how they will be distorted into rhomboids and ellipsoids respectively.

It will be remarked that every section of either the uncompressed, or the compressed, rhomboid, parallel to the shear, will be a square, equal to the section by the same plane of the original cube. And similarly every section so made of the uncompressed ellipsoid will be a circle, equal to the section of the sphere, and in the case of the compressed ellipsoid every such section will be also circular, and equal to the circle which is the section of the oblate spheroid, into which the given amount of compression would deform the sphere. Thus the sections parallel to the shear retain their original forms and dimensions, and this shows that fossils lying in that plane would not be distorted. Hence, since fossils in the plane of cleavage are distorted, and often greatly so, it follows that the plane of cleavage cannot be the plane of shearing. The explanation of this difference between a schist and a slate rock

appears to be, that the original rock which has been altered into a slate, was ductile, and more homogeneous, and the distribution of unhomogeneity, to which the cleavage is due, is caused by the distortion of the material, the component particles having been drawn out in the manner of the ellipsoid; and at the same time hardened by the normal pressure accompanied by some amount of condensation. This causes the principal cleavage to run parallel to the flatter faces of the ellipsoids of distortion; while the secondary cleavage runs along their sides[1].

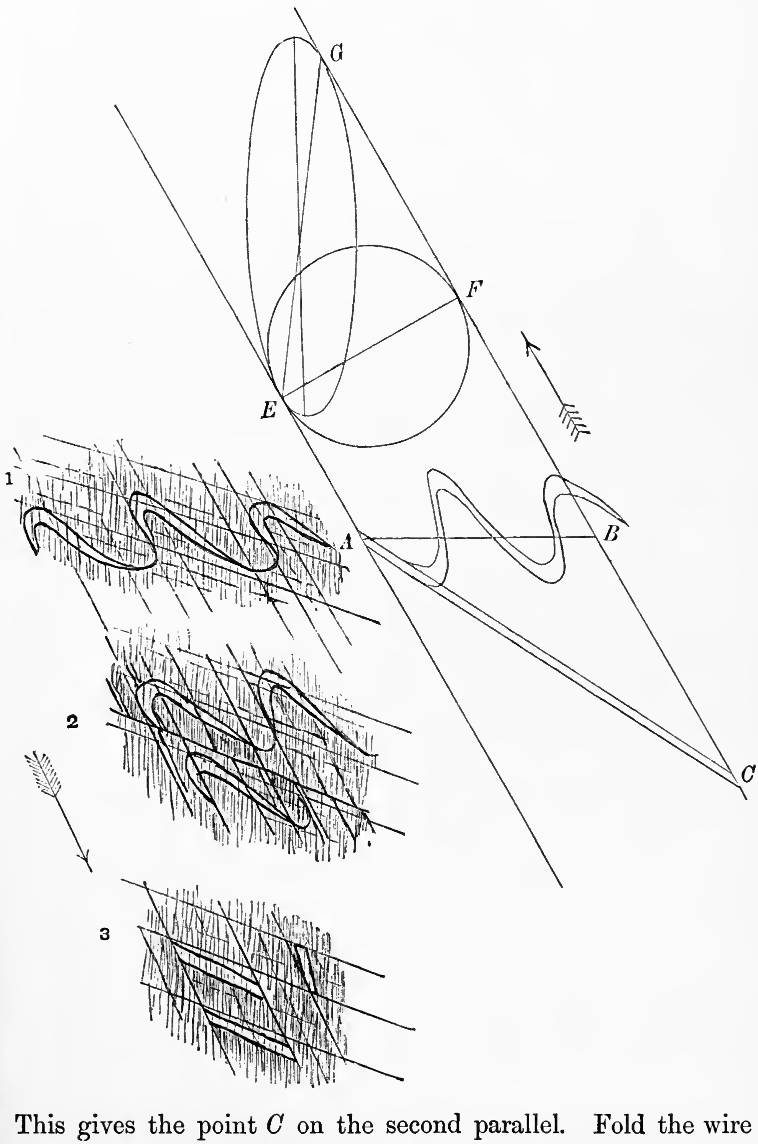
The diagrams 1, 2, 3, on the opposite page are reproduced with permission from a paper by Professor Hughes in the "Geological Magazine[2]." The two systems of parallel lines represent the more noticeable planes of shift, or shear, which have affected the rock. The three figures show the different effects produced by the same general movements at different places. The close shading represents the cleavage. It seems that the shear in the direction of the arrows has been the one, which has left its impress most markedly upon the structure. In (1) it has produced folding and cleavage. In (3) it has produced faulting, or thrust-planes, besides. The character of the folds is not seen in (3), but it appears that there have been folds, because a fragment of a layer in the right-hand top corner is turned into a quite different position from that of the layers seen in the middle part of the figure.

By laying a piece of fine wire along that part of the fold in (1), which is enclosed between the two extreme parallels, and then straightening it out, it appears that AC is the length of this part of the fold. Let us then make the following construction. Take the point A on the prolongation of the axis of the fold, and through A draw a straight line AE parallel to the direction of shear; and draw another parallel at the distance from AE that the extreme parallels in Professor Hughes' figure stand apart. Let AB be in the prolongation of the axis of the

[1] The Author has discussed the above subject in the "Geol. Mag.," Dec. III., vols. I. and II. The text gives his more matured conclusions.

[2] "The Lower Cambrian of Bethesda, N. Wales." "Geol. Mag.," N. S., Dec. III., vol. VI., p. 13, 1889.

fold in (1). Now lay the straightened wire in the position *AC*.



This gives the point *C* on the second parallel. Fold the wire again, and draw by it a snake-like fold, whose axis is *AB*.

This in the figure might have resembled the original fold more closely, but the principle is not thereby affected.

It is clear that, if shearing takes place, the layer *AC* will be folded into some form or another, the exact nature of which will depend upon the constitution of the matrix and of the enclosed layers, which will render it more shearible (to coin a word) along some planes than along others. If we draw imaginary lines parallel to the shear, and set off equal portions of them as ordinates to define the outside curves of the fold we shall get the correct thickenings and thinnings of the fold, which would be caused by one simple shear. These are best seen in the hypothetical diagram on page 265, but in the present instance they do not accord very closely with those given by Professor Hughes' sketch. Something may be due to the uncertainty of representation; but most probably the chief cause of the discrepancy lies in a secondary and subordinate shear, represented by the second set of parallel lines.

The point of chief interest however is the direction of the cleavage. With respect to this question it is evident that *CB* is the mean amount of shear. Draw *EF* perpendicular to the parallels, and make *FG* equal to *CB*. Then if we describe a circle on *EF*, it will be sheared into the ellipse *EG*; and the exact form of this ellipse has been given by making the ordinates equal to the ordinates of the circle on the same lines of shear. Hence if we draw its longer diameter it will give the position of the ellipsoid of distortion: and it is very remarkable how closely it agrees with the direction of the cleavage as drawn upon the spot by Professor Hughes: thus verifying our hypothesis concerning the mode of the production of cleavage in true slates.

The enormous amount of shearing, which some tracts of country have undergone, has been only lately appreciated[1]. In North West Scotland, a dislocation or shear from East to West occurs, the displacement of which is computed to be about 10 miles. In Norway one has been discovered which takes the

[1] See "Report on Recent Work of the Geological Survey in the North West Highlands of Scotland." "Journ. Geol. Soc.," vol. XLIV., p. 378, 1888. Also "Page's Geology" by Prof. Lapworth, art. 128 *et seq.* on "Recent discoveries among Metamorphic Rocks," 1888.

opposite direction, namely from West to East, with a dislocation of 100 kilometres. In the Alps similar "thrust-planes" occur, where the rocks have been sheared over one another for "scores of miles[1]."

(4) Besides corrugation and compression the strata of the earth's surface have been extensively affected by direct faulting. This has been usually, and probably correctly, attributed to the contraction and consequent shortening of the crust over those areas where it occurs; for by direct faulting a portion of the crust is enabled to occupy the same horizontal area after contracting, that it did before, without gaping fissures being formed in it.

It would seem that vertical rather than horizontal contraction (though the two would go on together) determines the course of faults. For horizontal contraction would tend to divide a country into polygonal areas like a bed of basaltic columns, but faults run in approximately straight lines. Now this is the direction followed by a crack which is formed by the depression of a lamina as may be observed in the case of ice. When one first steps on a sheet of ice that has never been disturbed it is usual to notice a crack run with a ringing sound, quite, or nearly, across the surface in a straight line from beneath one's feet. This is of the nature of a fault, and is another instance of the analogy between the behaviour of ice and the strata of the earth's crust.

We propose therefore to explain the phenomena of ordinary faulting by the occurrence of fissures due to contraction, commencing at the surface and running downwards; and among the causes which have given the first impetus to their formation may possibly have been the drying of a surface from which either the sea or inland lakes had been drained off, or even where the climate had changed and become arid. That some faults have originated on land surfaces appears from the observations of the United States Survey, who tell us that in the High Plateaux of Utah "The great displacements began in early tertiary time, and are probably yet in progress. The evidences of the

[1] Dr Tornebohm in a letter to Dr Geikie, "Nature," vol. XXXVIII., p. 127, 1888.

recency of some of these movements appear in the escarpments frequently seen along the line of faults where Quaternary beds have been broken at a time so recent that the escarpments have not been destroyed by atmospheric agencies, and further evidence is exhibited in the small amount of talus frequently found at the foot of a recently formed fault-scarp[1]."

The connection of faults with surface changes is indicated by the following passage from Captain Dutton's work: "It yet remains to speak of another interesting relation of the later system of faults. They have throughout preserved a remarkable and persistent parallelism to the old shore line of the Eocene lake following the broader features of its trend in a striking manner. The cause of this relation is to me quite inexplicable, so much so, that I am utterly at a loss to think of any subsidiary facts which may be mentioned in connection with it and which can throw light upon it[2]." It appears that the area of the Eocene lake has been raised by the faulting[3]. May not the removal of the superincumbent weight of water have been the proximate cause of the disturbance, and of the uprising of the lightened area?

Indeed we might expect that all such readjustments of the equilibrium of the crust as do not arise immediately from horizontal thrust would in general be effected by faulting. The features produced by this kind of movement would therefore be superimposed upon the features previously, or complicated with those subsequently, determined by compression and consequent corrugation.

The blocks into which the strata were cut up would on the whole follow the "lie" of the country as previously or concomitantly determined by the forces of compression, a general law thus lucidly enounced by Prof. Green: "We find that we can account for the observed facts only on one supposition, and

[1] "Report on the Geology of the High Plateaus of Utah, by C. E. Dutton, Captain of Ordnance, U.S.A. Prefatory note by Major Powell," p. viii. Washington, 1880. A description of perhaps the most magnificent system of faults in the world is to be found in Chap. II. of this work.

[2] *Ibid.* p. 45.

[3] *Ibid.* p. 38.

that is, that *the rocks have been folded into a series of troughs and arches, or thrown into domes and basins.* This is the great general law which governs everywhere the arrangement of the disturbed portions of the earth's crust. Faulting, or violent contortion and inversion, often complicate and obscure this structure and interfere with its symmetry, but never to such an extent as to prevent its being recognised as the great leading feature in the arrangement of the rocks[1]."

Faults occur of very different extent of throw, varying from a few inches to many fathoms, or even miles. In Captain Dutton's "Geology of the High Plateaus of Utah" we read of displacements by faulting of 5,500, 1,800, 12,000, 4,000, and 7,000 feet[2]. Prof. Bonney tells us that the perhaps largest known fault in the world is in the Appalachian Chain, where, on opposite sides of a crack over which a man can stride, beds are brought together that were once 20,000 feet apart[3].

These enormous faults are in a different category from the smaller "troubles," which are met with in mining and in ordinary geological mapping. These latter appear to be best explained by the contraction of the rocks on their becoming more dense, whereas the former appear to be connected with great uplifts of extensive areas, and to have been accompanied with volcanic outbursts. Such a fault might cut quite through the earth's crust. And indeed, where the throw is so great as to be measured by several thousand feet, it is scarcely conceivable that it should not do so, if our estimate of about 25 miles for the thickness of the crust is near the truth. The magma would then rise into it, and one of those great fissure eruptions would ensue, which have, at one or another geological epoch covered large portions of the globe with beds of igneous rock. The notion of a vast crevasse extending downwards to the fluid magma, and erupting at many vents, may seem a wild dream, and certainly has no parallel in anything known to

1 "Geology for Students and General Readers," 1876, p. 374.

2 Chap. II.

3 "Manuals of Elementary Science. Geology." S.P.C.K. 1874, p. 43.

occur at the present day and yet their former occurrence seems extremely probable[1].

Such are some of the phenomena for which we have to account.

[1] Richthofen's "Natural System of Volcanic Rocks," p. 17. "Mems. of California Academy of Sciences," vol. I., Pt. ii. San Francisco, 1868. Also "The Lava Fields of North-Western Europe," by Prof. A. Geikie. "Nature," vol. XXIII., p. 3, 1880, and by the same Author "The History of Volcanic Action during the Tertiary period in the British Isles." "Trans. Roy. Soc. Edin.," vol. XXXV., Part 2, 1888.

CHAPTER XXI.

VOLCANIC DYKES.

Compression not being sufficiently accounted for by contraction of the globe, hypothesis proposed that a cause of it is situated within the crust—Attraction of thickened crust not operative—Intrusive dykes widened by fluid pressure—Excess of horizontal over vertical pressure so caused—Dykes connected with volcanic vents—Example on the Colorado—Mineral veins—Hypothesis restated concerning the constitution of the magma—mode of its elevation in a fissure—Pressure on the sides of the fissure—Comparison of the work of compression with that done against gravity—Prof. Judd and Baron Richthofen on the relation of volcanic energy to continental movements—Possible additional compression upon solidification of dykes—Analogies of earth's crust and ice—Analogy of ice disturbed by skaters and earth's crust by sedimentation—Compression attributed to cracks opening from below, *and a continental phenomenon—Views of American Geologists as set forth by Dr Sterry Hunt—Effect of increased thickness of crust on compressing force—Whence the energy invoked—Other causes of compression yet to be sought.*

Our investigations in previous chapters have gradually led us away from the time honoured hypothesis, that the disturbances, which have at various periods affected the earth's crust, and of which we have given an outline in the preceding chapter, can be sufficiently explained by the settling down of the cooled crust upon a shrinking interior. The earth, being a cooling body, no doubt has shrunk, and that must have produced some effect upon the contour of the crust; and it is one among the causes, of which we are bound to take account. But we must assuredly seek for some additional agencies to do the amount of

work, which we see to have been done; for work there has been. It will not avail to appeal to original inequalities, for we know that there have been movements on a grand scale since the beginning of geological times.

We will first inquire whether any causes of compression may be found within the crust itself.

At first sight it might seem that, when the crust had been thickened in any region, the increase in the mass of the crust there through thickening might add to the attractive force of that region, and if the attraction was sufficiently powerful, might cause compression, by drawing the neighbouring parts towards itself. But the discussion of the attraction of mountain masses by Sir G. B. Airy, quoted in the fourteenth chapter, suffices to show that such a supposition would be erroneous. Indeed it is easy to see that, when a column floats vertically in a heavier fluid, the horizontal attraction upon a particle, at or near the surface of the fluid, will be somewhat less for a longer than for a shorter column. It is not possible therefore that the cause of compression can be explained by the attraction of the thickened crust upon the neighbouring parts, to say nothing of the weakness of this force.

In following up this new branch of the subject, let us inquire whether any geological appearances in the crust itself can give a clue to the cause of compression.

When we examine any district where considerable sections of metamorphosed strata are exposed, we are struck with the abundance of dykes of trap, and of similar intrusive rocks, which permeate them[1]. These are commonly, though not always, more or less vertical in position. Likewise in any section, carried across an extensive range of country, large and wide dykes are laid down, sometimes as actually determined by the surveyors, and sometimes as the supposed channels, through which over-

[1] Prof. Liveing, from an estimate by the eye alone, made at my request in the Channel Islands, thinks that the horizontal area occupied in the cliffs of Guernsey and Serk by intrusive dykes cannot be less than from 2 to 3 per cent. of the whole area. Jersey consists largely of volcanic products. See a paper by Prof. Liveing on the Channel Islands, "Proc. Cam. Phil. Soc.," part III., p. 122, 1881. He mentions a dyke in Serk 16 feet wide.

lying sheets of igneous rock have found their way to the surface.

What have been the conditions under which these intrusive more or less vertical sheets of matter have been brought into the positions in which we see them? That they have come up from below no one doubts; but what has been the force that has injected them? How have the chasms originated which they occupy? If the separation of their walls has been effected by any pressure acting within the chasms themselves, then we have an indication of the existence of some force tending to compress the crust.

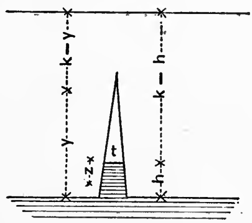
Upon the hypothesis that a crust of about 25 miles thickness rests upon a fluid substratum of fused rocky matter holding water-gas in solution, let us suppose that a crack is produced by any cause in the under surface of the crust. The pressure of the fluid at the bottom of the crust is that due to the weight of the superincumbent crust, and consequently the tension of the water-substance in the magma there must be the same. If therefore, upon the opening of a fissure which did not reach the upper surface[1], the magma beneath were able to fill it with water-gas[2], at a tension equal to the pressure which it sustains from the weight of the crust (about 10,000 tons upon the square foot), the fused rock could not rise into the fissure at all, but the sides of the fissure would be subjected to a gaseous pressure, only less than the pressure of the entire crust by the diminution upwards due to the decreasing weight of the superincumbent gas, which, compared with its tension at the bottom of the chasm, would be inconsiderable. Thus we should under the circumstances supposed (which could never be fully realised) have a horizontal pressure upon every elementary area of the sides of the chasm equal to the weight of a column of rock of the same sectional area and about 25 miles high.

Generally, let t be the tension of the water-gas which occupies the chasm, h the height to which the magma rises in

[1] Dr A. Geikie says that it is an observed fact that basaltic dykes do not always reach the surface, "Geological Sketches," p. 282, 1882.

[2] See Hannay "On the states of matter," "Proc. Roy. Soc.," vol. XXXII., p. 412. 1881.

the chasm, then, if we neglect the weight of the gas compared



with that of the magma, which owing to its high temperature we are probably justified in doing,

$$t = g\rho k - g\sigma h.$$

This will be the horizontal pressure upon the side of the fissure everywhere above the surface of the fluid magma in the fissure, while the horizontal pressure at a depth z below the same surface will be

$$t' = g\rho k - g\sigma h + g\sigma z.$$

At the same time the vertical pressure upon an element of the rock in the crust near the chasm at a height y above the bottom of it will be

$$p = g\rho (k - y),$$

which tends to cause the side of the chasm to bulge inwards.

Hence the difference between the horizontal and vertical pressures on such an element of the rock situated above the level to which the magma rises in the chasm, will be

$$t - p = g\rho y - g\sigma h.$$

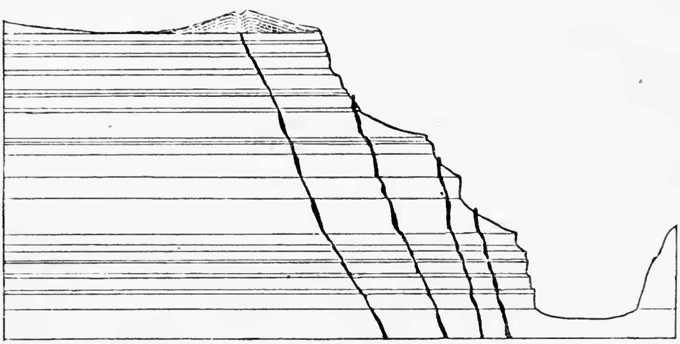
And the like difference below that level will be

$$t' - p = g\rho y - g\sigma h + g\sigma z.$$

The excess of the horizontal over the vertical pressure, which is the force that will cause lateral compression of the rock, increases as y increases, and also increases as h diminishes. This force will therefore be greater towards the upper part of the chasm, and it will also be greater the greater the tension of the vapour, which will tend to decrease h by preventing the magma rising. If the rock should allow the vapour only slowly to escape, and if the magma below the chasm can supply fresh

vapour sufficiently rapidly to keep up the temperature, it is conceivable that the tension of the vapour in the chasm may be nearly equal to $g\rho k$, in which case the magma will rise very little in the chasm, and the horizontal pressure will be much greater than the vertical, especially in the upper part of the chasm where y is nearly equal to k. We can therefore understand how a chasm, commencing below, may be rent open upwards, and at last reach the surface of the ground. When that occurs, vapour at a high tension will escape at whatever point is first broken through, and after issuing at a diminishing pressure for a certain time, it will be followed by the magma itself, which will overflow at the surface because the water-substance in the upper part of the column, expanding owing to the diminished pressure there, will render the whole column of less weight than an equal column of crust. This appears to be the mechanism required to explain the formation of a volcano, and of the earthquakes which precede its appearance[1].

The accompanying illustration is copied from Dutton's "Grand Cañon of the Colorado[2]." The trench, shown in section



at the right hand, is the Cañon, at the bottom of which the

[1] The above passage stands as in the first edition. It receives remarkable confirmation from the observation subsequently published, which is referred to in the following paragraph.

[2] "The Tertiary History of the Grand Cañon district," with Atlas. By Clarence E. Dutton, Captain of Ordnance, U. S. A., Washington, 1882. Reviewed by the Author, "Geol. Mag." Dec. II., vol. x., p. 318, 1883.

river flows. It is at this place half a mile deep. We see here in section four dykes of basalt. Three of them have had their upper parts, and whatever they may have ended in, cut away by the corrosion of the river. But the northern dyke terminates in a cinder cone, now standing at the extreme corner of the edge of the so-called "Esplanade." It is partly dissected away by the recession of the cliff on which it stands, and its structure exposed to view. "The very dyke through which the lava came up is disclosed to a depth of half a mile[1]."

It is probably to this kind of action that we may attribute the opening of fissures radiating from volcanic vents, along which subsidiary craters are established. Thus for instance an earthquake, accompanying a volcanic explosion in Iceland on the 4th January, 1875, "opened rifts forty miles in length in the plains north of Dýngjufjöll, and it was from fissures in a tract twenty miles in length and about three in breadth, which sank to some depth between two of these rifts, that the lava welled forth[2]."

If our chasm did not extend upwards to the surface, when the tension of the vapour in it diminished through cooling the lava would ascend further, until it became too viscous to rise higher. At such a juncture we might have vapours still at a very considerable pressure occupying the upper portion of the fissure, while the lower part would be filled with the magma, solidified at its upper surface and more and more fluid below. In this manner we may conceive gaping chasms[3] to have been formed, which might be for a long time occupied by heated vapours or hot water, and gradually converted into mineral veins.

If however the water-gas should not be able to escape from the magma sufficiently rapidly for the vapour pressure to accumulate so as to prevent the magma from almost entirely occupying the fissure, or if the fissure should open to the surface

[1] *Op. cit.* p. 95.

[2] "Volcanic History of Iceland," by Mr W. G. Lock, "Geol. Mag." Dec. II., vol. VIII., p. 214.

[3] The way in which boreholes in time become distorted shows that a chasm could not remain permanently open without internal support.

so that the vapour above the liquid should escape, then the magma would rise into the fissure pressed upwards by the weight of the crust around the base of the ascending column. In this case it would not behave within the fissure like an inert liquid, for it would carry with it the water-substance which is one of its constituents, and that, when raised within the fissure, would have an expansive force of its own. For this expansive force, necessarily equalised at the bottom of the crust to the pressure of the crust, arises from the store of energy due to the temperature of the water-substance, and to whatever place it may be transferred it will carry that energy with it. Indeed it seems almost certain that fissures filled with igneous rock must have been opened by the pressure accompanying its injection. For if the fissure had been produced by contraction of the "country," it would have been widened gradually, filling as it opened; and in that case the injected rock would bear traces of successive additions in layers arranged side by side. But a dyke usually consists of a homogeneous mass, the result of a single effort of injection.

In order to form some opinion as to the amount of this force, which resides in the ascending lava owing to its expansibility, we must consider what would be the behaviour of the magma after it had passed into the column, and assumed the condition which justifies the change of name to "lava."

We have already discussed the probable constitution of the magma. In a given mass of it we suppose a preponderating proportion of rocky matter, which is inelastic; and the remainder consists of gas, chiefly water-gas, at the same exalted temperature, dissolved in the fused rock in accordance with Henry's law. The presence of this gas causes the magma to expand when the pressure upon it is diminished, on account of the vesicles of gas which separate from it, and increase the mean volume. It is a matter of observation that in a volcanic vent this actually occurs, and produces ebullition. In arguing upon this question we have no means of knowing to what extent the volume of a given mass of magma would depend upon the amount of gas actually in solution, or "occluded." Probably the increase of volume from that cause is small, but on the

diminution of pressure the mean volume of the rock and gas together increases, so that eventually the lava expands into a mere froth or pumice, much of it, when it reaches the surface, being actually blown to pieces into an impalpable powder.

Let μ be the mean density of the vesicular lava under the pressure p. Then if σ' be the density of the rocky constituent only of the lava, and σ the density of the magma when the gas is all dissolved and there are no vesicles at all, since the density is inversely proportional to the volume, referring to our former work[1], and observing that σ now answers to $\sigma + \gamma\varpi$, we have

$$\frac{\mu}{\sigma} = \frac{d\zeta}{dz} = \frac{1}{1 + m\dfrac{\varpi}{p} - m}.$$

$$\therefore \mu = \frac{\sigma}{1 - m + m\dfrac{\varpi}{p}}.$$

In this formula ϖ is the pressure under which the gas is wholly dissolved. Regarding this to take place at the bottom of the crust,

$$\varpi = g\rho k.$$

Hence
$$\mu = \frac{\sigma}{1 - m + m\dfrac{g\rho k}{p}}.$$

To express the increment of pressure in the lava column corresponding to the increment of depth, let x be the depth below the surface of the crust.

Then
$$dp = g\mu dx,$$

$$= \frac{g\sigma}{1 - m + m\dfrac{g\rho k}{p}} dx.$$

$$\therefore \frac{dx}{dp} = \frac{1}{g\sigma}\left\{1 - m + m\frac{g\rho k}{p}\right\}.$$

[1] Chapter v., p. 58.

Integrating and observing that when $x = k$, $p = g\rho k$,

$$\frac{1-m}{g\sigma}(g\rho k - p) + \frac{m\rho}{\sigma} k \log \frac{g\rho k}{p} = k - x.$$

If p were determined from this equation, it would give the pressure in the lava at the depth x from the top of the crust; the column being supposed to be confined within the crust, and to fill the chasm. It would also give the pressure in a column of lava ascending above the crust into a chimney of infinite height, in which case the value of x, expressing altitudes above the surface of the crust, would have to be accounted negative: for it is obvious that, in the case of an infinite column, if the column was cut off by a diaphragm at any height, the portions above and below would remain as before separation; and this gives the case of a column within the crust, which does not reach the open surface. But it must be remembered that the equation expresses the condition of equilibrium, and could not be made applicable to the case of lava issuing from a chasm open at the surface, and in rapid motion.

In our investigation relating to Henry's law, we have seen that the supposition that the volume of the gas absorbed under any pressure by the molten rock, being between equality with and one half of that of the rock which absorbs it, will explain the absence of tides at the surface of the crust[1]. There being no appreciable tides renders it probable that this supposition is correct, and it is expressed by giving m some value between 1 and $\frac{1}{2}$.

First let $m = 1$.

This will reduce our equation to the simple form,

$$\frac{\rho}{\sigma} k \log \frac{g\rho k}{p} = k - x,$$

whence
$$p = \frac{g\rho k}{\epsilon^{\frac{\sigma}{\rho}\frac{k-x}{k}}}.$$

[1] p. 62.

At the surface of the crust $x=0$,

and
$$p=\frac{g\rho k}{\epsilon^{\frac{\sigma}{\rho}}}.$$

If we give to σ and ρ the values 2·96 and 2·68 respectively, then $\epsilon^{\frac{\sigma}{\rho}}=3·0176$ and the pressure at the surface of the crust is one-third of that of a column of rock of the height and density of the crust. We may feel assured that the density of the substratum does not differ very much from that of the crust; so that $\epsilon^{\frac{\sigma}{\rho}}$ cannot be much larger than 3, and this estimate is fairly reliable. It shows that, if the conditions are such that a column of lava is working its way upwards through the crust, its crown is pressing upon the rock above it with a powerful thrust, and we see the reason of such a phenomenon as occurred before the eruption of Santorin in 1866, when the sea bottom was gradually lifted up into an elongated dome, from which blocks of rock fell away as it rose, until red-hot masses were protruded, and eventually the melted lava itself appeared[1].

But if m has any other value than unity, the equation does not admit of such a simple form. When $x=0$, the relation at the surface of the crust will be,

$$\frac{1-m}{m}\left(1-\frac{p}{g\rho k}\right)+\log\frac{g\rho k}{p}=\frac{\sigma}{m\rho}.$$

And if we put m equal to $\frac{1}{2}$, we find by trial that,

$$p=\frac{g\rho k}{4·241}.$$

This shows that, if the volume of gas absorbed under pressure is only half that of the molten rock, the pressure at the top of a column reaching just to the surface will be still very considerable, bearing to the pressure in the case where the volumes are equal the ratio of 3 : 4. So that in this case also

[1] See Fouqué's "Santorin."

similar phenomena of uplifting, such as occurred at Santorin, might be expected.

Under the same circumstances we may find the whole pressure on the side of the chasm, for it will be

$$\int p dx = \int p \frac{dx}{dp} dp$$

$$= \int \frac{1}{g\sigma} \{(1-m)p + mg\rho k\} dp$$

$$= \frac{1-m}{g\sigma} \frac{p^2}{2} + m \frac{\rho}{\sigma} kp + C.$$

If we assume that $m = 1$ and put for p its corresponding value in terms of x this becomes

$$\frac{\rho}{\sigma} g\rho k^2 \left(\frac{1}{\epsilon^{\frac{\sigma}{\rho} \frac{k-x}{k}}} + C' \right).$$

For the whole column reaching to a depth x below the top of the crust this has to be taken from x to k and the pressure upon the side of the chasm is

$$\frac{\rho}{\sigma} g\rho k^2 \left(1 - \frac{1}{\epsilon^{\frac{\sigma}{\rho} \frac{k-x}{k}}} \right).$$

If the column reaches just to the top of the crust (but does not flow out) the whole pressure will be

$$\frac{\rho}{\sigma} g\rho k^2 \left(1 - \frac{1}{\epsilon^{\frac{\sigma}{\rho}}} \right).$$

If we give to ρ and σ the usual values this becomes

$$0{\cdot}905\, g\rho k^2 \times 2/3.$$

Hence the whole pressure upon the side of the chasm will be

$$\frac{3}{5} g\rho k^2.$$

The mean pressure will therefore be $\frac{3}{5} g\rho k$, or three-fifths of the weight of a column of rock of the height of the thickness of the crust.

If the chasm was filled with inert liquid of the density of

the crust it is clear that it would exercise no compressive force. The whole pressure in that case would be $\frac{g\rho k^2}{2}$. Hence the whole compressive force of the lava which arises from the expansibility of the gas will be $\left(\frac{3}{5}-\frac{1}{2}\right) g\rho k^2$ or one-tenth of the weight of a column of the density and height of the crust upon the side of the chasm. This will give $2\frac{1}{2}$ miles' pressure as the average compressing force acting upon the side of a chasm filled with liquid lava but not escaping at the surface, such pressure being caused by the expansible gas mingled with the lava.

If we again assume that the volume of the gas absorbed is one half that of the molten rock, then $m = \frac{1}{2}$, and the whole pressure on the side of the chasm will be found by taking

$$\frac{1}{2}\frac{p^2}{2g\sigma} + \frac{1}{2}\frac{\rho}{\sigma}kp + C$$

between the limits which p has at the top and bottom of the crust, *i.e.* between $\frac{g\rho k}{4 \cdot 241}$ and $g\rho k$, whence giving $\frac{\rho}{\sigma}$ the usual value $0 \cdot 9054$ we find the whole pressure to be $0 \cdot 56 g\rho k^2$.

This is $0 \cdot 06$ greater than the pressure of a column of the density of the crust. Hence $0 \cdot 06\ g\rho k$ is the mean compressing force exercised on the side of the chasm by a column of lava, reaching to the top of the crust but not escaping, upon the supposition that the volume of gas absorbed is one half that of the fused rock which absorbs it. It would be a pressure equal to the weight of a column of the crust one and a half miles high. In the case where the volume of absorbed gas was supposed equal to that of the molten rock, we have just seen that the pressure would be that of a column of crust two and a half miles high.

It has previously been proved[1] that the work against gravity expended in raising an elevated tract of length l will be

$$g\rho\frac{\sigma}{\sigma-\rho}\,hl\bar{y},$$

[1] p. 170.

where $\overline{y}$ is the height of the centre of gravity of the elevated tract, and h its mean height. We see then that this part of the work, when the volume of the elevated mass is given, varies as the height of its centre of gravity, and this includes the work of depressing the root of the tract into the substratum.

Again, if t be the mean pressure upon unit width of the side of a chasm of height k, and the chasm be opened through a space λ by the action of t, then the work done by t will be $t\lambda k$. But since the opening of the chasm gives rise to the elevation above and the depression below the mean levels, it follows from equality of volumes that,

$$\lambda k = \frac{\sigma}{\sigma - \rho} \int y\,dl$$

$$= \frac{\sigma}{\sigma - \rho} hl.$$

Hence the work done by the compressing force is

$$t\frac{\sigma}{\sigma - \rho} hl.$$

Whence we have,

$$\frac{\text{work done against gravity}}{\text{work of compression}} = \frac{g\rho\overline{y}}{t}.$$

To form some estimate of the work capable of being thus effected, let us consider the case of a tract of uniform height h; and then we shall have $\overline{y} = \frac{h}{2}$.

If the compressing force arises from the chasm being filled with liquid lava which is confined above, then we have seen that, with the assumed values of ρ and σ, when $m = 1$, $t = g\rho k/10$. And when $m = \frac{1}{2}$, $t = 6g\rho k/100$.

Putting $\frac{h}{2}$ for $\overline{y}$, and substituting these values, we find in the first case,

$$\frac{\text{work done against gravity}}{\text{work of compression}} = \frac{5h}{k};$$

and in the second case, $$= \frac{100h}{12k}.$$

Hence if there was no work required to deform the rocks, the whole being expended against gravity, we should have in

the first case $h = 5$ miles: and in the second case, $h = 3$ miles. We may say therefore that the intrusion of lava into a chasm, upon the two hypotheses we have made respecting the solubility of the gas, would be competent to raise a tract to a uniform height of five miles and of three miles respectively if gravity alone had to be overcome, but not to a greater height than this at any place.

This height, it must be remembered, is reckoned from the upper mean level of the crust, which is below the sea level. But when we consider the height of many mountain chains above the sea level, it seems clear that the cause of compression now under consideration would not be competent to have raised them, especially when account is taken of the work necessary for deforming the rocks. Nevertheless it does not follow that this cause of compression is altogether inoperative; and we may fairly conclude that volcanic action, in the form of the injection of lava into an elongated fissure, would be competent to produce a certain amount of elevation and distortion of the rocks.

That this must be really the case, is apparent from the width of many igneous dykes being greater than can be fairly attributed to the mere contraction of the "country" in which they occur[1]. And if the intruded lava has widened the chasm which contains it, compression and elevation must have been the result of its intrusion; and when we consider that, with time given, all known substances yield more or less to deforming forces, it seems quite possible that this may be so. And the facts which we shall bring forward presently[2] make it almost certain that some of the work has been effected in this manner. Moreover the rocks becoming fissured from contraction, will detract from their rigidity, and render shearing more easy, when large volumes of them are under consideration.

When the molten rock injected by hydrostatic pressure into the fissures came to solidify into the dykes which so abundantly

[1] That the intrusion of igneous rock is accompanied by expansive pressure is shown by laccolites, and intrusive horizontal sheets like the whinsill. For no contraction of the rocks of the "country" could produce horizontal chasms. See Gilbert's "Geology of the Henry Mountains," Washington, 1877.

[2] pp. 293—5.

intersect metamorphic strata, it is probable that another efficient cause of compression would come into play, for, as already mentioned, the result of recent experiments appears to show that such substances as whinstone and granite are less dense in the solid than in the liquid state at the melting temperature and must therefore swell on freezing. In this respect then they behave like water.

If this fact be established then it appears that pressure lowers the melting temperature of such rock masses; and consequently, if it be true that any part of the interior of the earth is rigid, this condition cannot be due to pressure, unless the deep-seated materials differ from those which have yielded these experimental results.

There are points of analogy so close between the circumstances of ice resting upon water, and such a crust as we have supposed resting upon a liquid substratum, that we may expect to receive suggestions for explaining the phenomena of crust movements from the behaviour of ice. In both cases the floating substance is but slightly less dense than that upon which it floats. In both cases the one is incorporated with the other by congelation. In both cases the floating substance has a considerable amount of rigidity, but yields somewhat freely to differences of pressure when sufficient time is allowed. These analogies have been noticed by many[1].

The ice upon a sheet of water that has been much skated on, after a few days' frost assumes an appearance very much akin to that of a moderately disturbed area of the earth's crust. This no doubt arises from the cracks which are formed, and by day are filled with water, which during the night freezes and expands, ridging up the ice on both sides of them. The transference of sediment from one locality to another has an effect analogous to the irregular distribution of load by the skaters' movements, and produces cracks opening upwards from below, carrying out the analogy.

[1] Prof. Nordenskiöld gives a description of the condition of the ice seen by him in travelling across the frozen sea to the north of Spitzbergen, and deduces conclusions from it regarding movements in the crust of the earth. "The Arctic Voyages of Adolph Erik Nordenskiöld," 1858—1879. London, Macmillan, 1879, p. 239.

But besides the unequal pressure caused by local deposits of sediment, which must tend to crack the crust from below upwards, the mere contraction of sedimentary rocks, when they sink into hotter depths by becoming overloaded, and there become metamorphosed and assume a denser character, must cause them to crack during the process. It is to the opening of cracks *from below* and the subsequent intrusion of lava that we now venture to attribute some part of the corrugation of tracts of the earth's surface. It necessarily follows that this action is the consequence of changing conditions, and is to be looked for where sediment is being deposited, and sedimentary rocks are becoming metamorphosed as along the shores of continents, rather than beneath the quiescent depths of the profound oceans, where there is but little deposition of sediment. Thus we are again led to regard compression as a continental phenomenon.

Prof. James Hall, Dr Sterry Hunt, Prof. Joseph LeConte and other American geologists connect the exhibition of compression of the crust with those regions of the earth's surface where thick sedimentary deposits have been previously laid down. Thus they conceive the corrugation of the Appallachian chain to have been a consequence of the extremely thick series of deposits of nearly the same age, of which that range consists. Dr Sterry Hunt[1] considers that "The effects of heat and water upon the buried sediments would be *condensation*, from the diminution of porosity and still more from the conversion of the earthy materials into crystalline species of higher specific gravity, thus causing *contraction* of the mass. A further and very important result of this accumulation was by the softening of the underlying floor, or the '*bottom strata to establish lines of weakness or of least resistance in the earth's crust, and thus determine the contraction which results from the cooling of the globe to exhibit itself in those regions, and along those lines where the ocean's bed is subsiding beneath the accumulated sediments.*' Hence, I added, 'we conceive the subsidence invoked by Mr Hall, though not the sole nor even the principal cause of the corrugations of the earth's strata, is the one which deter-

[1] "American Journal of Science and Art," May, 1861 (II., XXXI., 411).

mines their position and direction by making the effects produced by the contraction, not only of sediments but of the earth's nucleus itself, to be exerted along the lines of greatest accumulation'"[1].

The theory so epitomised appears to have gained very general acceptance among physical geologists. But if the reasoning of the earlier chapters of the present volume is correct, the cooling of the globe cannot be appealed to to furnish the requisite compression (in the above extract called "*contraction*"), much less would it furnish a sufficient balance of compression along a belt of crust, itself contracting through metamorphic action. But upon the view now suggested, the circumstances fit in very well. The thick deposit depressing the crust, induces metamorphism in its lower portion. This cracks in consequence. The magma fills those cracks with highly expansive lava, and a certain amount of compression is the result.

It is also important to remark, that the mean lava-pressure in a chasm would be increased when the crust was thickened, because the tension at the bottom of the crust is proportional to its thickness. On this account, although it would require a greater horizontal force to compress the crust where it is thicker, yet the requisite increase would be furnished to the force by the very fact of the increased thickness. If our theory of the formation of volcanic vents be accepted, it will be also apparent that the force which opens them will for the above reason be proportionally greater where the crust is thicker, so that their occasional occurrence on lofty plateaux is no argument against the theory.

Although from what has been said it appears that the intrusion of the molten magma into fissures, though capable of exerting considerable horizontal pressure and producing some amount of compression, would not be efficient to raise any of the higher ranges, nevertheless it appears that there has been a general connection between volcanic activity and the elevation of mountains. Professor Judd shows that the Alpine chains have been raised contemporaneously with

[1] Sterry Hunt, "American Journal of Science and Art," 1873, Art. XXVIII. p. 265.

the manifestation of strong volcanic outbursts along lines parallel to them[1]. He appears to adopt in general what we may call the American theory of the formation of mountains just mentioned, but he supplements it by specially connecting volcanic outbursts with the actual throes of their elevation. "The movements which resulted in the crushing and crumpling of the thickened mass of sediments along the Alpine line of weakness, also gave rise to the formation of a series of fissures from which volcanic action took place[2]." It is here the synchronism to which we desire to draw attention. Thus again: "We have abundant evidence that just at the period when these great movements were commencing which resulted in the formation of the great Alpine and Himalayan geanticlinal, earth-fissures were being opened upon either side of the latter[3] from which volcanic outbursts took place. At the period when the most violent mountain-forming movements occurred, these fissures were in their most active condition, and at this time two great volcanic belts stretched east and west, on either side of, and parallel to, the great Alpine chain."

Of the same purport are the remarks of Baron Richthofen on the connection between the distribution of volcanic rocks and elevated regions of the surface. He asks, "Was the particular structure of certain portions of the crust of the globe, which is indicated by the situation of elevated regions on its surface, among the causes of the eruption of volcanic rocks? or were the inequalities of level on the surface, in the volcanic regions, due to the processes attending and the agencies causing the eruptions? The answer to both these questions must be in the affirmative[4]."

The same author writing of the mode of geological occurrence of the rocks which have formed massive or fissure eruptions, says of andesite that "preeminently the greater part

[1] "Volcanos," Chap. x.

[2] *Ibid.* p. 297.

[3] The word "latter" strictly should confine this assertion to the Himalayan range. But from the enumeration of the localities, most of which are European, this cannot be intended. *Ibid.* p. 298.

[4] "Natural System of Volcanic Rocks," p. 82.

of it has to all appearance been ejected through extensive fissures; that is, it has been produced by what are styled massive eruptions...... Andesitic mountains are characterised by monotony in scenery. They form continuous ranges which are often of considerable elevation and extent[1]." Again of this rock he tells us that in Hungary, "andesite composes entirely the Hargitta-range, which extends over one hundred miles in length and twenty-five in width; the Vihorlat-Gutin-range, which is of still larger dimensions, and the Eperies-Kaschau-range; all of which are densely wooded, and of a gloomy monotonous aspect[2]." Similarly he mentions belts and ranges of basalt[3].

According to the suggestions offered in the present chapter to explain one cause of compression of the crust, the store of energy from which the mechanical force is derived resides (1) in the heat of the interior, and (2), if we refer any part of it to expansion of the rocks on solidifying, in the molecular force attending a change of state; while in the theories discussed in the former chapters the force is derived from the gravitation of the crust towards the centre of attraction of the globe. This latter force is practically infinite compared with the work it would have to perform in shearing the crust, but it has been shown that the limit to the space through which it can have acted, owing to the small thickness of the crust compressed and the small amount of the globe's contraction, is so quickly reached, that it can go no appreciable way towards explaining the phenomena. The case is different with regard to the theory now suggested. Here the force (1) is limited, since it never can reach the pressure of a column of the earth's crust, viz. about 10,000 tons upon the square foot, but there is no limit to the space through which it might act, so long as the fissures were sufficiently numerous and did not reach the surface.

It would however be a mistake to suppose that, having found one cause which may with great probability have been efficient to a certain extent, no other can have cooperated with it. In all physical problems there is a danger of holding one

[1] *Ibid.* p. 75. [2] *Ibid.* p. 30. [3] *Ibid.* p. 27.

theory of causation to the exclusion of others that are compatible with it. It is likely that several causes have contributed to produce those movements of compression which are so general, and which indeed appear to be more extensive and more energetic than the mode of action now suggested can suffice by itself to explain.

CHAPTER XXII.

THE VOLCANO IN ERUPTION.

Early opinions regarding the seat of volcanic energy—Hopkins' lava lakes—Mallet's theory—Opposed to the results of the present work—His mode of estimating the temperature derivable from crushing rock—His results published in the Philosophical Transactions—Localization of heat from crushing not possible—Theories of Scrope and Dutton—Local and temporary increase of temperature by conduction not probable—Richthofen's theory—Objections to it—Explanation of volcanic phenomena on the hypothesis of a liquid substratum holding water-gas in solution—Eruption, how brought to an end—State of intermediate activity—Unsympathetic craters of Hawaii—Richthofen's objections to the presence of water-substance answered—Prof. Prestwichs' theory of agency of water—Steady welling out of lava—Rising currents beneath volcanic areas—Comparison of volcano with geyser—Varying composition of lavas from the same vent.

It has been taken for granted more than once in the preceding pages, that the seat of volcanic energy is situated in a liquid substratum, which we have shown reason to believe must underlie the cooled crust of the earth. This is the natural supposition, and was formerly generally assumed by geologists. But when Hopkins published his supposed proof of the great thickness of the earth's crust[1], he was constrained to offer a fresh explanation of this class of phenomena. Hence his theory of subterraneous lakes of lava. When Sir Wm. Thomson corroborated Hopkins' view, and carried it further to the extent of asserting the entire solidity of the globe as a whole, the old assumption concerning the universal distribution of liquid matter beneath the crust received a further blow. The

[1] p. 37.

theory of Hopkins' lakes was permitted to stand, but it was felt to be so improbable, that physical geologists in general could not rest satisfied with that explanation.

At this juncture Mallet published his theory of volcanic energy[1] which at the time of its publication was received with considerable favour by many. Impressed with the necessity of admitting the doctrine of a solid earth upon the authority of the highest masters of physical science, they saw in Mallet's hypothesis a way of escape from the difficulty in which they were placed. If the earth be a solid body cooling by conduction, and if the corrugations upon its surface are caused by the contraction of the mass and consequent compression of the superficial layers, what, it was thought, can be more in accordance with probability, than that the work of compression should be converted into heat, and that volcanic energy should be its manifestation, and have its seat no deeper than the compressed crust. Like many simple explanations, the only wonder was that no one had suggested it before. Yet how often had been long delayed the discovery of some theory, which when once proposed seems simplicity itself.

If Mallet's theory be true, it is obvious that that now propounded must be untenable. It will therefore be necessary to examine it.

The theory was enunciated in the following terms[2]:—

"*The heat from which terrestrial volcanic energy is at present derived is produced locally within the solid shell of our globe by transformation of the mechanical work of compression or of crushing of portions of that shell, which compressions and crushings are themselves produced by the more rapid contraction, by cooling, of the hotter material of the nucleus beneath that shell, and the consequent more or less free descent of the shell by gravitation, the vertical work of which is resolved into tangential pressures and motion within the thickness of the shell.*"

As regards the lateral pressure to which Mallet appealed as the cause of compression and of crushing portions of the shell, there can be no doubt that, if there were a continual

[1] "Phil. Trans. Royal Soc.," vol. CLXIII, p. 147, 1873.
[2] *Op. cit.* § 67, p. 167.

contraction of the interior and subsidence of the shell going on, the pressure arising from the descent of the shell would be perfectly adequate to produce the crushing attributed to it.

We have however, as we believe, proved the insignificant amount of this contraction, which could not possibly produce any appreciable movements of the crust during such periods of time as history embraces. Yet volcanic phenomena are of daily occurrence. This appears to dispose of the theory *in limine.* Nevertheless it is possible that this argument against it may not meet with general acceptance, and it is therefore desirable to examine Mallet's theory upon his own assumptions.

No one can study the paper, in which the theory is propounded, without admiring the amount of knowledge displayed in it, and the numerous and laborious experimental investigations which he undertook to form the basis of his theory. The records of these alone will render his work of lasting value, and we have freely used, in the preceding pages, his results. Nevertheless we cannot avoid the conviction, that they do not warrant the conclusions which he drew from them.

The experiments which form the foundation of the theory may be shortly thus described:—

Cubes of rock of sixteen different kinds, varying in respect of hardness from soft oolite to hard porphyry, were carefully cut into cubes of $1\frac{1}{2}$ inches on the edge. These were then crushed by means of a powerful lever, under a cylindrical piston or plunger of $3\frac{1}{2}$ inches diameter. The cubes of rock appear not to have been confined at all laterally, so that, when they gave way, they were compressed into a cake of powder beneath the plunger.

The pressure upon each square inch of the face of the cube was calculated from the accurately known pressure laid upon the plunger, and the vertical descent of the plunger, while the crushing was going on, was also carefully measured. These multiplied together gave the "work" of crushing, and by dividing the work so obtained by Joule's equivalent the corresponding quantity of heat was calculated, it being assumed that all the work was transformed into heat.

If we consider the summary of the series of experiments in

crushing cubes of rock[1], it is evident that the vertical descent of the plunger must have been rendered much greater by the cube of rock having been left free to fall asunder on all sides. If it had been confined in a box of its exact size it might still have been crushed; but the amount of descent would have been in that case dependent solely upon the increase of density which could be given to it after disintegration by the pressure. If the box had been somewhat larger, the plunger would have descended further, and if the rock was altogether unsupported, as seems to have been the case, further still. The value of H (the heat) found in accordance with the experiment is correct; but the form of the experiment cannot represent at all closely what would happen deep in the earth's crust. It seems that the cubes should have been confined, that the experiment might more closely represent the case of nature.

But accepting the results as given, the equation connecting the quantities involved may be thus arrived at. Let

W = the pressure laid upon the plunger.

h = the height through which the plunger descended.

J = Joule's equivalent, or the number 772.

H = the number of units of heat into which the work of crushing was transformed.

Then $$H = \frac{Wh}{J}.$$

Also, t = the temperature through which the rock was raised by the crushing.

s = the specific heat of the rock, *i.e.* the number of units of heat requisite to raise 1 lb. of rock through 1°F.

$$= \frac{\text{quantity of heat requisite to raise } w \text{ of rock through } 1^\circ\text{F.}}{w}$$

$$\therefore \quad ts = \frac{\text{quantity of heat requisite to raise } w \text{ of rock } t^\circ}{w}$$

$$= \frac{H}{w}.$$

Hence $$t = \frac{H}{sw}.$$

[1] *Op. cit.* Tab. I. column 19, p. 187.

This is Mallet's equation (6), § 102, where he takes w to be the mass of one cubic foot of rock, and of which we have for clearness given a demonstration. His experiments gave for the mean value of the specific heat of rock the number 0·199; and for the mean weight w of a cubic foot of rock 177 lb. The mean number of British units of heat developed by crushing one cubic foot of the harder rocks is estimated by him at 5650; and it appears upon calculating the value of t, from his experiments that the mean temperature by which a cubic foot of such rock would be so raised is 172° F. Or if we take the particular kinds of rock selected by Mallet (§ 133), these means are found by him to be 6472 and 183°·74 F. And if the rock was previously at 300° F., taking 2000° as the fusing temperature, he finds 0·108, or rather above one-tenth, as the fraction of a cubic foot of rock which the heat developed by crushing *one* cubic foot of rock could fuse. Or, to put it otherwise, it would have required the heat developed by crushing *ten* volumes of rock to fuse about *one*[1]. But no reliance can be placed upon these estimates on account of the excessive value attributed to h.

Accepting the conclusion for the sake of argument that the heat resulting from crushing one cubic mile could fuse 0·108 of

[1] Here we meet with a remarkable discrepancy between the results given in the paper in the "Philosophical Transactions" and those previously given in the introductory sketch to Mallet's translation of Palmieri's "Vesuvius," for it is there stated that using the mean of 6472 units of heat as derived from crushing one cubic foot of rock "each cubic mile of the mean material of such a crust, when crushed to powder, developes sufficient heat to melt 0·876 cubic miles of ice into water at 32°, or to raise 7·600 cubic miles of water from 32° to 212°F., or to boil off 1·124 cubic miles of water at 32° into steam of one atmosphere, or, taking the average melting point of rocky mixtures at 2000° F., to melt nearly three and a half cubic miles of such rock, if of the same specific heat." It is obvious that it makes no difference in the ratio, whether the unit be a cubic mile, or a cubic foot, or a cubic inch. So that the statement here amounts to saying, that the crushing of one cubic foot of rock would melt 3½ cubic feet, instead of 0·108 of a cubic foot; or more than 32 times as much as subsequently stated in the "Transactions". Indeed the experimenter must have felt surprised that the cubes were not melted, seeing that he had estimated the heat which would be developed as three and a half times as great as would be necessary to do it. There is a foot-note to the paper in the Transactions which speaks of a modification of § 102. This may be connected with the discrepancy referred to.

a cubic mile, since this is not capable of fusing the whole of the cubic mile crushed, Mallet considered that this heat may be localized, and that the heat developed by crushing ten cubic miles of rock, might fuse one mile. But it may be conclusively shown that this is not possible; for let us consider a horizontal prism of rock of any length. This is itself a part of the earth's crust, and by its rigidity has, up to the moment of its giving way, resisted, and so permitted, the necessary accumulation of the pressure which eventually causes it to yield. Conceive that in this prism there are portions situated here and there which are weaker than the average, and that these weak portions when crushed allow of the prism being shortened at the places where they are situated by the quantities α_1, α_2, α_3, &c. respectively. It is clear that the weaker places will yield first and under a less pressure, and by the relief so afforded delay the crushing of the others, because the pressure must accumulate afresh. But, for argument's sake, we will suppose all to yield together, and the pressure throughout the action to be equal to the value it had at the first yielding.

Now suppose that when the prism has yielded the whole of it becomes shortened by the length a. If, then, P be the pressure which caused it to yield, Pa will be the whole work done upon the prism. The length a is made up of the portions α_1, α_2, α_3, &c., by which the weak portions have been shortened; while $P\alpha_1$, $P\alpha_2$, $P\alpha_3$, &c. are the portions of work done at these places. And these taken together make up Pa, since

$$\alpha_1 + \alpha_2 + \alpha_3 + \&c. = a.$$

We see, then, that the work must be confined to these places; for if there were work done elsewhere we should have more work than Pa, which is impossible. Hence the work convertible partially into heat takes place at all these places, and at each in proportion only to the yielding, and nowhere else; so that it cannot be localized at any one place. We may then conclude that, unless the heat got out of crushing any portion of rock is sufficient to fuse that particular portion, none will be fused. Indeed we may go so far as to assert that, if Mallet's experiments give the amount of heat which can be obtained by

crushing rock, and the heat so obtained can fuse it, then the cubes experimented upon ought to have been fused.

In the former edition, and more fully in the "Philosophical Magazine[1]," calculations were given to show that, under the most favourable circumstances conceivable, Mallet's crushing could not evolve heat enough to account for volcanic phenomena. But since the theory seems to have lost its former influence among geologists these are now omitted.

It will be noticed that Mallet's theory of volcanic energy suggested a mode of producing periodically by mechanical means local increments of temperature beneath volcanic areas, temporarily fusing the rocks, and causing their eruption. It was the mechanical part of the theory which was novel, for Scrope appears to have surmised some kind of periodical fusing[2]; and Captain Dutton has more recently suggested, "that the proximate cause of eruptions is a local increment of subterranean temperature, whereby segregated masses of rocks, formerly solid, are liquified[3]." But neither of these writers attempts to explain the cause of the increment of temperature. Such a rise of temperature can be produced only in one of three ways; (1) by mechanical work being locally converted into heat, or (2) by local chemical reaction, or (3) by heat being transferred from some hotter neighbouring region. The first is Mallet's hypothesis, and is believed to be insufficient. The second was Davy's, and has long been abandoned[4]. There remains only the third. Now there are but two ways in which heat can be in such a case conveyed from hotter to cooler regions, and these are by conduction and by convection. If conduction be the process involved, then we must have an unfused mass of rock beneath, and adjoining, the area affected, at a temperature higher than is requisite to fuse other rocks; which is in itself improbable. But that such a condition of excessive tempera-

[1] Fourth series, vol. L, p. 302, 1875 (in which occurs the following erratum. At p. 316, line 6, insert k, for "$1\frac{1}{3}$ mile" read "533 miles," and *dele* the following line). See also Mallet's reply, fifth series, vol. I. p. 19, and the Author's rejoinder, *Ibid.* p. 138.

[2] "Volcanos," p. 263.

[3] "Geology of the high plateaus of Utah," p. 120. Washington, 1880.

[4] p. 3.

ture should be local and temporary, is still more so, because the tendency of conduction would be to equalize the temperature along isogeotherms approximately parallel to the outer surface.

There is, however, the theory of Richthofen to be considered. Instead of invoking a rise of temperature, he supposes a temporary increase of fusibility. He appears to think that there are heated couches of rock, in a condition which he calls "glowing lava[1]," but which are not in a state of fusion. Fissures being formed in the crystalline crust above this, water gains access to it, and it enters into a state of "aqueous fusion," and thereby becomes increased in volume, and "this expansion would immediately cause a motion of the masses rendered liquid, in the direction of least resistance, that is, upwards in the fissure, and would, if continued for a sufficient time, make the same overflow on the surface of the crust, even if unassisted by other ejecting agents, such as the vapour of water[2]."

The difficulties attending the accession of superficial water to deep seated volcanic foci have already been pointed out[3], and it seems hard to believe that the mode of action here described can have a real existence. Moreover Richthofen appeals to the cooling of the globe as the ultimate cause of disturbance, of which cause we claim to have proved the inadequacy. He says, "elevations and eruptive activity, even when locally not quite coincident, are co-ordinate effects of the cooling of the globe; but while the one is its immediate effect, the other results from it only by the concurrence of other agencies, which by themselves alone would have been incapable of producing results of such magnitude." The memoir from which these passages are taken is of great interest, and contains many very suggestive observations, and sage generalisations well worthy of study. On the whole his theory of volcanic action approaches more nearly than any other to that which we now advocate.

The existence of convection currents beneath the cooled crust of the earth at once furnishes a means of obtaining those

[1] "Natural system of Volcanic Rocks," "Memoirs of the California Academy of Sciences," vol. I. part II. 1868, p. 66.

[2] *Ibid.* p. 56. [3] p. 143 *et seq.*

local increments of temperature, which in some form or another appear to be needful to explain volcanic phenomena. The results arrived at in chapter VI show conclusively that, if the crust of the earth is no thicker than the greater number of geologists conceive it to be, there must be active currents of this nature. It is equally clear that these currents must bring up liquid against the under side of the crust at a temperature somewhat above the temperature of solidification. The conduction of heat through the crust in a general way suffices to carry off this access of heat somewhat more rapidly than it is supplied, so that the crust on the whole grows slowly thicker; but it is obvious that if at certain times and places the hot magma should be supplied with exceptional rapidity, the result would be, that the crust would there be thinned off, and the heated magma brought more near the surface than elsewhere. It does not seem necessary that these places should always be situated above the head of main upward currents; for a horizontal current meeting with inequalities such as the roots of mountains might surge backwards, and bring lower and hotter strata of the liquid to the top. So little is known about the laws of convection, or indeed of fluid motion in general, that it is not easy to predict what the play of such currents might be. But at any rate we seem to have found in them the knowledge of a source of local increments of temperature.

There is besides the other mode by which the liquid magma may be expected to find access to the surface. Changes in the distribution of load by transference of sediment will have a tendency to produce fissures, and it has been pointed out how these, commencing below, may be propagated until they reach the surface, and become filled with lava, in which the expansion of the water-substance, at a temperature far above the critical, will render the column of molten rock on the whole specifically lighter than the mean crust, so that it will escape at the surface.

In a general way this seems probable; but we have already shown that, under certain admissible suppositions, what we have just stated will necessarily occur. It is true that we have considered the problem only as a statical one, for the ascent of

vesicles of water through the lava, causing ebullition at the surface, has not been taken account of, but merely the decrease in density of the lava upwards, caused by the expansion of the water-substance in it through decrease of pressure, the same identical water being supposed to remain always engaged in the same portion of lava[1]. But ebullition would help to raise and eject the lava.

Henry's law teaches us, that the volume of gas under pressure, which the molten rock can absorb, is always the same: in other words, when the magma under a given pressure is saturated, the mass of gas in solution will be proportional to the pressure. But the magma need not be always and everywhere saturated. It can always hold a smaller quantity than the maximum in solution. And since the gas, although dissolved, exercises an elastic pressure, the elastic pressure of the magma will vary with the amount of gas dissolved in it. It is probably to this that the well known fact is due, that a long-continued state of repose in a volcano during which the gas has had time to accumulate is followed by an exceptionally violent eruption, and *vice versâ*. Consider what must happen during an eruption. In order that the pressure at its base may bring the lava to the surface of the crust, it is requisite that the mean density of the column should be lowered by a certain amount of water being vesicularly mingled with it. But the vesicles rise through it, and eventually leave the column of lava deficient in enough water-substance to enable it to reach to the surface. At the same time the magma around the base of the funnel has been to a certain extent exhausted of its superabundant water, and thus an eruption will have the effect of diminishing the proportion of water in the magma. Exceptionally powerful eruptions might be expected to occur when a volcano had been quiescent for a long while, and the water-gas had had time to become again diffused, or else fully saturated fresh magma had been brought by currents beneath the vent, and the gas thus regained its normal elasticity at the place. Or when a volcano has newly burst through the crust

[1] p. 283.

at a place where the water-gas had never, or at least not for a long period, been drained away, an unusually violent explosion might take place.

The observations of Sir I. Lowthian Bell[1] upon slags of the iron furnace, and of Captain Dutton upon lava currents, prove that fused silicates can hold a notable amount of gases in solution even under the atmospheric pressure, and extrude them with considerable violence upon solidification. These facts must have an important bearing upon volcanic action. During the secular refrigeration, and consequent thickening, of the earth's crust, the gases, which were previously dissolved in the magma, must be extruded as it slowly solidifies. These gases cannot be redissolved in the subjacent magma if it is already saturated. They must therefore either find their way upwards in the form of free gas, so much of it as consists of water gas ultimately becoming condensed into steam and hot water, when it reaches levels whose temperature is below the critical; or else, they must accumulate at high tension, like air in the top of a diving bell, in places where there may happen to be hollows in the under side of the crust, such as probably exist beneath old habitual vents, or along the course of great dislocations, where one wall dips deeper into the substratum than the other. The gases which so accumulate might be partly or wholly discharged in the form of steam and gas by solfataric action. If wholly, the volcanic energy of the locality would be kept in abeyance, if only partly, it would accumulate, and after an appropriate interval assert itself with more or less explosive violence, in proportion to the obstruction by which the gases had been confined. We may conceive that by this means a crater of explosion might be suddenly formed, as happened on the 15th of July, 1888, when, "almost in the twinkling of an eye, Little Bandai-san was blown into the air, and wiped out of the map of Japan. A few minutes later its *debris* had buried or devastated an area about half the size of London[2]."

There would always be a tendency, by means of diffusion from the general substratum, for the magma at any place to

[1] "Journal of Iron and Steel Institute." No. II., 1881.
[2] "Times," 11th and "Nature," 13th Sep. 1888.

become saturated; and the period of an eruption might depend upon the time requisite for this to be accomplished, because, until then, no fresh accumulation or pressure could commence beneath a vent, from which the gas had been recently drained away, for we can hardly suppose the currents to move quickly enough to repair the loss in a period commensurate with the duration of an ordinary volcanic eruption.

When an eruption is in progress, and the lava has found free access to the atmosphere, the vesicles have freedom to escape, and the exhaustion of gas in the column goes on more rapidly than diffusion can repair it. Consequently the lava in the upper part of the column at last becomes so deficient in vesicles of gas, that its weight balances the expansive force beneath it. The vesicles beneath it will then cease to expand, and their upward motion will be checked, and the evolution of steam gradually almost cease. But the column of lava has been for some while rising in the vent, and at this juncture the forces which press it upwards are just in equilibrium with those that keep it down. The momentum which it already possesses will however be only gradually destroyed, and it will continue to well forth steadily and without much evolution of vapour for a little while and then sink back into the vent[1]. In this manner the eruption will be brought to a close.

Instances are recorded in which an eruption of lava has been followed, instead of preceded, by a great evolution of steam and ashes. It is conceivable that vapour may accumulate in parts of the chasm which are closed above, and when the level of the lava which confined it there has sunk sufficiently, steam may find an exit by the open vent, rushing with violence through the lava, now no longer deep enough to restrain its escape, and

[1] Since the above appeared in the first edition, the Author has met with an example evidently depending on this kind of action. At Mr Chaston's steam flour-mill at Shelford near Cambridge they have a well which is dug to the depth of a few feet, and bored further for 260 feet. The supply not being sufficient to cause the bore-pipe to overflow, they inserted a suction pipe some way into the bore-pipe. When the pump is at work it lowers the water in the bore-pipe, and its surface oscillates to the stroke of the pump. But as soon as the pump is stopped, the water rises to the top of the bore-pipe, and overflows for a short time and then sinks back again.

blowing it into dust; the mode of action being somewhat similar to that which was proposed to explain the phenomenon of a geyser before Bunsen suggested a better.

Upon our hypothesis of the presence of water as an original constituent of the magma, if so much water-gas must be present in the column to produce an active condition, we can understand how moderate quantities of the intensely hot gas passing upwards, although insufficient to cause a strong eruption, may nevertheless be enough to keep the column of lava, or at least its axis, in a state of fusion: and it must be remembered that heat would escape but slowly from rock-surrounded lava. But if the means of supply of water-substance be more liberal, and be just balanced by the means of escape, a gentle and perpetual state of eruption like that of Stromboli, may result. That the maintenance of the temperature of fusion in a habitual vent is due to the cause suggested, appears probable from the following consideration. Molten lava sometimes fills the bottom of a crater for a long period, its surface being maintained in a constant state of ebullition by the vapour and gases which pass through it. In such a case it must be the high temperature of these vapours which keeps it melted: for if the fusion were due to a continual supply of fresh lava from below, there would need to be a continual escape of it above. But this does not appear to be requisite. If such be a correct view of the case, it shows that the hot vapours are capable of fusing rocky matter, and accordingly, when once they have obtained a passage of escape, they may convert the sides of the channel into lava and fuse their own way through, carrying the fused matter with them[1].

It has been sometimes urged that volcanic vents cannot communicate with a common reservoir below, because, even in the same mountain, contiguous craters are not sympathetic. But this sympathy does not seem really necessary, for as soon as a column of ascending vapour has established itself in one crater, the tendency will be for it to continue to hold the same

[1] See p. 46: also the author's paper "On the Inequalities of the Earth's Surface upon the hypothesis of a liquid substratum." "Camb. Phil. Trans.," Feb. 1875.

position, since its presence renders that the vertical of least pressure, and consequently the channel of easiest escape for the gas, at the same time that it establishes an upward convection current in the liquid. We see the same sort of thing take place when water begins to boil in a vessel heated below. Before it passes into general ebullition, a column of bubbles will appear here and there, and rise from the same points of the bottom of the vessel for a considerable time. So, when the escape of vapour has commenced through one of the craters, it may carry off the vapour as fast as it is supplied, and determine the eruption to that channel, the neighbouring crater remaining quiescent, although they may both of them be in free communication with the same subterraneous reservoir[1]. The level of the lava in the crater which is in eruption will be raised above that in the other, because the column beneath it is rendered lighter by the expansion of the increased quantity of superheated water which is passing through it at the time. The case will be somewhat like that of an inverted syphon, the liquid in one leg of which is lighter than that in the other.

In the crater of Kilauea molten lava has been sometimes emitted from clefts in the upper parts of the vertical cliff, which surrounds the Black Ledge, and at a level some hundreds of feet higher than the open surface of the great lava pool below; and has run down on to the Black Ledge. In this case it is probable that the lifting of the lava to so great a height is due to the narrowness of the channels, which gives the gas no opportunity to escape until it reaches the top, carrying the lava up with it; whereas in the pool below it can get free at the much lower level of the wide and open surface.

The explanation now offered for the elevation of the lava to the surface, differs from previous theories of the same kind, on account of the supply of water-substance being drawn from the

[1] Another explanation has been offered that the two great craters of Kilauea and Mouna Loa, which although on the same mountain are not sympathetic, may be connected with different fissures, which is not improbable, seeing that they are twenty miles apart. The diagram of dykes and a cinder cone on page 281 favours this explanation. After the eruption of 1843 from the summit of Mouna Loa, Kilauea, though 10,000 feet lower in level than it, showed no change whatever in its condition.

entire liquid substratum, and not from the access of it to the lava at some point higher up. The importance of this distinction appears from an objection made by Richthofen[1]. "It must here be remarked that some of the most eminent writers on the subject of volcanoes, chiefly Poulett Scrope, Prof. Dana and others, have suggested long ago, that the ascent of lava in a volcanic channel must be due to both the fluidity and expansion imparted to it by the globular state of the water which finds ingress to the channels of the lava and enters into its composition. They have anticipated, by this suggestion, in some measure, the results of experiment established by Daubrée. But the supposition was only made for the case of lava, and not for that of rocks which were ejected without volcanic action proper" (that is by massive, or fissure eruption). "Yet even in regard to volcanoes it did only explain the extension of lava to the surface from a place at a limited distance below it, and failed to give a clue to the manner in which the constant supply of matter to those places was kept up[2]." Both of these objections appear to be met if the entire liquid substratum is laid under contribution for the supply of the water-substance which acts as the intermittent agent in raising the column to the surface.

Professor Prestwich thinks, that the immense quantity of steam usually given off during an eruption is derived from springs or the sea, and that it is conveyed along the strata through which the lava rises: the water being as it were sucked in by the ascending column. But he does not explain whence the motive power comes, which causes the lava to "rend and crash through the plug," as he aptly expresses it[3]. Moreover it seems hardly probable, that there should be heat enough to spare in the molten rock to vapourise so large a quantity of water without itself becoming congealed[4]. The

1 "Natural system of Volcanic Rocks," "Memoirs of the California Academy of Sciences," vol. I. Part. II. 1868.

2 *Ibid.* p. 54, note.

3 "On the agency of water in volcanic eruptions," p. 150 of reprint from "Proc. Roy. Soc.," read Apr. 16, 1885.

4 This appears to have actually happened in the eruption of Mt. Tarawera in New Zealand, in 1886. The eruption took place at a succession of vents along

strongest argument alleged against the presence of water as an original constituent of the magma is, that instances are on record of lava quietly welling forth without the evolution of steam. We may reply, that it is difficult to make sure, from the accounts we get, of what really does occur during an eruption. Unless it can be shown that lava overflows the lip of a crater, without being preceded by an evolution of steam, this would be no valid argument against our theory; because if a pool of lava within the crater were to be tapped by an orifice opened in the side of it the lava which flowed out would carry no free gas with it, but only so much as it might happen to hold in actual solution. All the free gas would escape inside the crater, and not through a lateral opening, for bubbles do not move horizontally. The flow from the lateral opening once established, would continue to well out so long as the supply lasted[1]. Is it certain, that the accounts we have of steady welling out, do not apply to lateral outflows?

The mode of action suggested appears competent to account easily for the eruption of steam and lava in a case where a channel of communication has been freshly opened between the liquid substratum and the surface. There is more difficulty in understanding how, when an eruption has taken place, and the water-

a fissure nine miles in length. This fissure ran in a nearly straight line, paying no regard to the contour of the surface. Where it intersected Mt. Tarawera, the lava appears to have been so charged with water-gas, that it was all blown into fragments, and there were no coulees. But it is certain that liquid lava was present, because the sides of the chasm were splashed and plastered with it. The water-gas which blew the lava into ashes, must therefore have come up with it. Where however this fissure intersected Lake Rotomahana, there were no fiery manifestations. The waters of the lake seem to have congealed the rising lava, and the eruptions consisted only of steam, mud, and stones. "Report on the eruption of Tarawera and Rotomahana, N.Z." By A. P. W. Thomas, M.A., F.L.S., p. 47. Wellington, 1888.

[1] The following instance is from Scrope's "Volcanos," p. 160. "In the later eruption [of Etna] in 1792 Ferrara observed that a fissure was broken through the side of the mountain, whence, during ten days the lava boiled out very tranquilly, while the aëriform explosions took place only from the principal crater. At the end of this time the explosions ceased from the main crater, and commenced from the extremity of the fissure, at the same moment that the lava ceased to flow out; the liquid column within the vent having evidently lowered itself, by continual emission, to the level of the lateral aperture."

substance has been exhausted to that degree at which the lava becomes too dense to rise any longer, the action can be after a period renewed. But the considerable size of volcanic areas occupying, as they do, large extents of country, dotted over with points of eruption both ancient and modern, leads to the conclusion, that the liquid magma must be abnormally near the surface beneath such areas. The presence of convection currents in the substratum would render this possible, for such areas would only need to be situated where the crests of currents or rising eddies impinge upon the under side of the crust. This would cause the thickening of the crust to go on more slowly, and at times alterations in the play of the currents would cause the refusion to outstrip the freezing, and the crust would be as it were washed thin. Then, as shown in Chapter XXI.[1], an eruption would be imminent[2]. When it took place, the evolution of gas accompanying it would soon reduce the upper part of the magma to an inert condition, while the abstraction of heat by the escaping gases and vapours would reduce its temperature, and the vent would become more or less closed. What would be required therefore to repeat the eruption, would be the diffusion afresh of sufficient water into the column for that purpose, and it seems that the escape of water-gas from the surface becoming reduced in amount beyond the supply gained by diffusion or convection from below, must be the requisite condition. This may be perhaps caused by the cooling of a plug of lava at the top of the column, except a narrow passage through which what escape there is takes place, and which would serve as the leading channel for the next eruption. Or else, when the period of repose has been protracted until this channel has become blocked with cooled lava, some fresh movement of the crust might open the necessary communication between the surface and the fluid substratum.

The comparison between the geyser and the volcano is no new one. Bunsen's explanation of the former turns upon the temperature at each depth in the column of water becoming

[1] p. 286.

[2] This seems to have been the condition of the district of the hot lakes of New Zealand when Mount Tarawera was split open.

raised above the boiling point for the pressure there. Our account of the condition of the lava is somewhat similar, except that the boiling point of the lava depends, not so much on the temperature, as upon the presence of a sufficient quantity of water mingled with the lava, to cause the requisite expansion of volume under the pressure at each depth in the column.

It is easily seen, that the presence of currents in a liquid substratum will go far to account for changes of mineral character from time to time in the lava issuing from the same volcano. Under such circumstances, we cannot reason upon the constitution of the lava, as if it was always derived from a local reservoir of limited extent, whose contents remain constantly of the same composition: nor yet as if it came from a magma arranged in layers, strictly in accordance with the specific gravity of their component matter.

CHAPTER XXIII.

GEOLOGICAL MOVEMENTS EXPLAINED.

Recapitulation of amount and suggested causes of compression—Probable duplex character of corrugations—A new hypothesis proposed—Currents in the substratum—Stress on crust from the same—Vertical uplifts—Suboceanic volcanos—The consequences of denudation followed out—Elevation its correlative—Fresh-water strata covered by marine—Movements most energetic near, but not confined to, continents—Degradation of mountains a law of nature—Enormous thickness of certain strata accounted for—Raised sea beaches—Two classes of elevatory movements—Drainage across dip—Theories of compression compared.

In Chapter XIX. we have seen reasons for believing that compression has probably been confined to continental areas, or at least has not affected the submarine crust beyond a margin not very far distant from the coast. We have also concluded that, calculating from the existing elevations and making the hypothesis of approximate hydrostatic equilibrium, this compression is probably not less than four per cent. of the linear dimensions. Moreover in Chapter XX. we have described the great amount of shearing by which older deposits have been thrust on to the top of newer, the displacement in Scotland amounting to over ten miles and in Norway to much more, the movement in both instances being away from the ocean. Similar displacements have occurred in the Alps. Such is the character, and such the scale of magnitude, of the phenomena of which we seek the explanation.

In our endeavour to discover the cause of this compression,

our results hitherto obtained have been mostly negative. But even negative results have great value, because the disproof of a theory, which for want of close examination may long hold its ground, removes a chief obstacle to further advance in knowledge. It is submitted, then, that the theory has been disproved (1), That the earth is a solid body cooling by conduction, and that the inequalities which appear on its surface have been caused by the contraction of the interior through cooling.

It has further been shown that geological phenomena require us to suppose that the crust of the earth rests upon a fluid substratum, and this belief has led to the examination, and rejection, of a second theory, (2) That the crust is thin, and is so far flexible that the fluid may rise into the anticlinals formed by the corrugations of the crust; a view held by some who have written on the subject[1], and at one time entertained by the Author himself. The rejection of this hypothesis has led to the supposition that the crust is flexible only to a small degree, and that, under compression, the corrugations which are raised upon the surface must be accompanied by corresponding downward protuberances of larger dimensions projecting into the fluid below, thus causing them to be duplex: and it has been pointed out that certain facts, brought to light during the great Indian survey and showing that the attraction of mountainous regions was much less than might have been expected, were better explained by Sir G. B. Airy upon the supposition of such downward protuberances, than in any other way that had been suggested. We have also proved in Chapter XV. that anomalies in the variations of gravity as determined by the pendulum are likewise explicable on the same hypothesis. Moreover we have shown that certain other facts of an entirely different class, connected with the observed slow rate of increase of temperature beneath mountains, are also better accounted for in this manner, than in the way originally proposed by the observer, who studied and described the phenomena most carefully.

These two circumstances appear to lend a very considerable

[1] See note, p. 254.

amount of support to this view of the subject, so that the diagrammatic sections of Chapter XIII. which represent the outcome of it, are thus far corroborated.

The Author at one time suggested a third hypothesis. It was, (3) That the compression, which produced the corrugations, has arisen from a diminution of the earth's volume through extravasation from beneath the crust of the water now composing the oceans. Possibly this may have had a slight effect. But, considering how little the volume of a given mass of fused rock would be likely to be increased by the absorption of the water it could hold in solution, the result must be inappreciable, as is shown in Chapter XIX.

A further suggestion has been put forward in Chapter XXI., namely, that the rise into a chasm of lava derived from the magma of the substratum would produce sufficient pressure upon the walls of the chasm into which it was injected to compress the crust adjoining. This was found upon calculation to be competent to give rise to some amount of compression: but it can hardly be thought sufficient for the whole result which we see to have been produced.

The investigations of Chapter VI. have however led to the discovery of a circumstance, which appears to afford a new, and perhaps sufficient, explanation of the greater movements of the earth's crust, which geologists have sometimes called "crust-creep," and have attributed to an unknown action which they have termed "earth-pressure." The argument stands thus. Geological phenomena suggest to us that the crust is thin, and that it rests upon a liquid substratum. It is shown in Chapter V. that the objection to this hypothesis, based upon the tides, is not necessarily fatal. We therefore feel justified in accepting it. But mark the consequence of doing so, which is disclosed in Chapter VI. There we learnt that, if the crust is thin, it is certain that the liquid on which it rests must be in motion. Currents must lave the underside of it to hinder it from too rapidly thickening, dissolving it away somewhat less quickly than it freezes. If this were not so, in the lapse of geological time the crust would have become so thick, that it could not exhibit the phenomena, which even at the present day point to

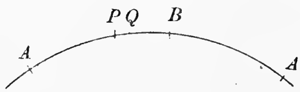
its being comparatively thin. Since the effect of these currents has been to delay freezing, they must bring heat from below, so that, by whatever cause they are started, they must be of the nature of rising convection currents.

Still further we have been led by the calculations given in Chapter XVII. to believe, that the densities of the external layers of the earth, in the continental and oceanic areas respectively, are arranged in such a manner, that the substratum will be less dense beneath the oceans than beneath the land. This implies that the oceanic areas are situated above the ascending currents, which will therefore impinge upon the underside of the suboceanic portions of the crust. Since the liquid cannot descend again at the same place where it ascended, but must move for some space more or less horizontally, we here come at once upon a mode of action tending to press the supernatant crust from the oceanic towards the continental areas where the descending currents will be situated.

This would afford a cause of compression tending to produce coast ranges, which, as we have pointed out in Chapter XIII., would be most steep on the side from which the movement came—that is on the side fronting the ocean. It is impossible to tell with certainty what the exact behaviour of such currents would be. We do not know what the exact form or size of the areas may be which are occupied respectively by rising and descending currents. But since the exciting cause viz. the cooling of the interior underlies the whole, it seems probable that the cooling agency which resides in the rising currents will at any given period affect the larger area. This will cause the descending currents to move the more rapidly of the two. The liquid must move to some extent horizontally between them, and it may help us to form a general idea of the effects which currents would have in producing compression if we make such a supposition as the following.

Suppose that a rising current impinges upon the underside of the crust at A, and, flowing along towards B, descends again at B. Then the horizontal velocity of the liquid is zero at A, and thence increases up to a certain amount, and then decreases

until it again becomes zero at B. Measuring along the underside of the crust let $AP=x$, $PQ=dx$, $AB=a$.



Now let us suppose for argument's sake that the horizontal velocity of the current, that is its velocity along AB, is expressed by $u \sin(\pi x/a)$. This will make the horizontal velocity zero at A and B, and give a maximum velocity u half way between. We now wish to find the stress in the direction of the crust, which this current may produce. For this purpose we suppose the crust rigid. The action of the current upon the crust will be of the nature of fluid friction. The motion being slow, we will assume it to be proportional to the velocity. Then the stress produced upon the element PQ, to urge it in the direction PQ, will be

$$\mu u \sin\frac{\pi x}{a}\,dx.$$

Hence the whole stress produced upon AQ will be

$$\int \mu u \sin\frac{\pi x}{a}\,dx = -\mu u\frac{a}{\pi}\cos\frac{\pi x}{a} + C.$$

At A, where $x=0$, the stress is nothing.

$$\therefore C = \mu u\frac{a}{\pi}.$$

Hence the stress on AP

$$= \mu u\frac{a}{\pi}\left(1-\cos\frac{\pi x}{a}\right),$$

$$= \mu u\frac{a}{\pi}\operatorname{versin}\frac{\pi x}{a}.$$

The whole stress on AB if it did not yield, but transmitted the stress throughout its length, would be $2\mu u\frac{a}{\pi}$

It will be necessary to suppose another upward current at some place A', to meet the current coming from A, so as to produce the downward current at B. Then the point B in the crust will have no tendency to move horizontally. But it seems

probable that the currents from A and A', meeting at B, would tend to heap up some of the liquid under B, and by that means to raise the crust there. If the rising currents were due simply to a higher temperature, and the descending ones to a lower, this might not occur. But if, as seems likely, the upward currents are due to vesicles of expanding gas, that gas would not travel far horizontally, and the descending currents would be due to a heaping up of a portion of the liquid above the level of general equilibrium where the horizontal currents met, rather than to a greater intrinsic density of the liquid. Thus there might arise elevation of the crust at B owing to an access of liquid producing direct upward pressure on it.

The horizontal stress at P is caused by the accumulated action of friction along AP, and requires that AP should be strong enough to support the stress without being crushed. Under the conditions we have imagined the stress becomes greater the further P is distant from A.

When the distance from A is so great that the accumulated stress becomes more than the crust can withstand, compression will take place, and the full amount of stress will not be transmitted further. Accordingly, although our provisional formula gives the greatest value for the stress as occurring at the point B, it does not follow that it will in fact be greatest there, because, if the crust yields, say, at P, the stress accumulated along AP will be expended in compression at P, and will not be fully transmitted beyond. It will however begin to accumulate afresh after P has been passed.

In our provisional expression for the stress, viz.

$$\mu u \frac{a}{\pi} \operatorname{versin} \frac{\pi x}{a},$$

μ measures the fluid friction between the liquid and the crust. It would be small. Also, u being the velocity, is probably small. But a may be some hundreds of miles and is therefore large. As versin $(\pi x/a)$ increases with x, at a considerable distance from A the stress may become large enough to produce the effects we have attributed to it, in spite of μ and u being small. It is also to be observed that although we have taken u as

constant it must in fact increase as the distance from A increases because additional liquid rises all along under AP.

The conclusion at which we have arrived is that the currents, which, if the crust is thin, must exist in the substratum, will tend to compress the crust locally at some distance from mid-ocean. What that distance may be will depend conjointly upon the velocity of the current, the frictional resistance between the liquid and the crust, and the strength of the crust. The frictional resistance will be largely affected by the contour of the underside of the crust. It seems certain that downward protuberances would offer a marked resistance to the flow, which would give the horizontal currents increased power to compress the crust where they occurred. Thus mountainous regions would always be especially exposed to horizontal stress. It seems also that, where thick deposits had been going on, the depression of the crust into the substratum resulting from their weight might at last attain such dimensions, that the impulse of the currents upon them would eventually produce compression there; and that the American theory, that mountain ranges have been formed out of thick deposits, might thus receive an explanation.

In estimating the effect of the impact of horizontal currents upon downward protuberances of the crust, it must be borne in mind that, although the movement is probably slow, yet the density of the liquid is great. Their momentum would be about three times as great as if the liquid was water. It is by the action of these currents that we would now propose to explain the major part of the compression, which has at all periods affected the continental crust, producing contortion and disruption of the rocks. We have also pointed out how the meeting of such currents may by accumulating liquid possibly elevate an area by a vertical uplift.

Once more to make use of the analogy of ice, one may notice how, when cakes of ice float down a river and impinge upon the edge of an icy barrier, they get partially pushed up over it, and piled one upon another, exhibiting an arrangement not unlike that of strata dislocated by thrust planes. Here the operative cause is the friction of the water flowing beneath, and

catching a hold upon the edge of any cake tipped down into the influence of the stream. It seems reasonable to suppose that a somewhat similar cause, owing to the friction of the substratum, has acted in the case of rocks sheared over one another, as in the Highlands.

Let us now consider what would happen beneath the oceans, where the ascending currents impinge. The liquid tending to spread laterally will produce a tensile stress in the central parts, which will become converted into a compressive stress as the continental areas are approached. In the central parts we may therefore expect that the crust will be fissured, and that volcanic eruptions will be the consequence. This may explain why so many volcanic islands are found in mid ocean, and why so many eruptions take place in the bed of the sea, even where no permanent volcanic islands are formed. It is evident that whatever amount of compression is caused by this kind of action in the continental areas, must have its correlative extension in the width of fissures beneath the oceans, which will become dykes of igneous rock in the suboceanic crust.

The instability of convection currents, which shift their positions from slight disturbing causes[1], will go far to explain the instability of the earth's crust; for there must be slight changes of level produced by the ascending and descending currents,—slight that is as compared with the dimensions of the larger inequalities of the surface such as the height of mountains and the depth of oceans.

We will now trace somewhat further than was done in the thirteenth chapter, the results of denudation and of additional compression upon an elevated tract supported in approximate

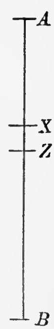
[1] This instability may be illustrated by a simple hypothetical case. Suppose a horizontal layer of liquid with patches of different temperatures, within a mass of liquid otherwise of uniform temperature. Ascending currents would start over the warmer patches, and after partial cooling at the surface descend again upon the cooler patches. Eventually the temperature would be equalised, and the currents ought to stop. But the momentum of a current keeps it moving after the exciting cause has ceased, and when friction at last stops the motion, the process has been carried too far, and the relative positions of the warmer and cooler portions reversed, so that currents are started in new relations to each other. Convection in the substratum would of course be of a more complicated nature.

hydrostatic equilibrium, and compare them with natural appearances. It is evident that, as the upper and exposed part is degraded, the equilibrium will be roughly speaking restored by the whole mass being lifted up, so that the part above the effective level of the fluid should hold approximately the same ratio as before, that of $\sigma - \rho : \rho$, to the part below it. Consequently, before a mountain chain can be completely levelled down, not only must the originally elevated portion be degraded, but the crust beneath must also be lifted up and brought under the influence of the denuding agents, possibly down to the neutral zone, and the whole of the material so removed converted into sediment, and transferred to some lower level[1].

It is not however necessary to suppose that the roots of the elevated tract ever at any one time bore this proportion of $\rho : \sigma - \rho$ to the whole volume which has ultimately been denuded away; for this would not be the case unless the compression and accompanying elevation were due to a single effort. For if, after a part had been denuded off, and the roots of the mountain had been concurrently lifted up, a second period of compression had ensued, the neutral zone would then have had its position along a lower line of particles, and the

[1] To determine to how great a depth denudation might be expected to expose the crust, suppose that A is the crest of a corrugation, and B the bottom of the corresponding protuberance, Z the place of the neutral zone; and let X be the point that is ultimately brought to the surface by denudation. We have concluded that, probably,

$$AZ = \tfrac{2}{5}AB, \text{ and } ZB = \tfrac{3}{5}AB.$$



Now if X is ultimately brought to the surface, B will at the same time be brought to the lower mean level. Hence

$$XB = c.$$

And therefore $$AX = AB - c = \tfrac{2}{5}AB + \tfrac{3}{5}AB - c$$
$$= AZ + \tfrac{3}{5}(AB - 5c);$$
$$\therefore\ AX - AZ = \tfrac{3}{5}(AB - 5c).$$

Hence if $$AB < 5c\ AX < AZ,$$

that is the neutral zone will not be brought to the surface unless $AB > 5c$.

But AB is about 11 times the height of the mountain.

Therefore the height of the mountain must have been $\tfrac{5}{11}c$, or say 10 miles, before denudation commenced, in order that the neutral zone should come to the surface.

depression produced might not have been so great at the second as at the first effort. The same may be said of a third, or of any subsequent effort. In fact, according to the ordinary reasoning of the method of limits, we may, by diminishing the intervals and increasing their number, pass to the case of a continuous movement of elevation, growing less and less until it ceased altogether; denudation going on all the while at a greater rate than, except at first, material was brought up for it to act upon.

If the tract were now to become free from further compression, the sequence of events we have sketched out would be terminated by its being reduced in thickness approximately to the normal thickness of the crust in its neighbourhood. But its internal structure, as exhibited in sections, would betray the results of the treatment to which it had of old been subjected. It would form a nearly level tract, consisting of the lower parts of the folds of highly contorted rocks, which had been subjected to the action of a temperature possibly above the critical temperature of water, and to great pressure.

The sediment, which had been transferred from the tract thus denuded down, would some of it have probably been spread over low lying land, and some of it have gone out to sea, and formed marine deposits not very far from the shore. Both these would have sunk nearly as fast as they accumulated, and by this means unmetamorphosed strata would have been depressed into regions where their temperature would have been gradually raised, and metamorphism set in. This would render them more dense. They must necessarily contract in consequence, and those results follow which in the preceding chapter we have attributed to the intrusion of lava into cracks opening upwards. Compression might result and act directly upon the mass itself, or it might be communicated to a neighbouring tract; but on the whole it seems probable that the mass itself would be the first affected.

The suggestions now put forward appear to give a fairly satisfactory explanation of the causes of the deposition of series of marine strata of great thickness, of unconformity, of metamorphism, of contortion and elevation. The phenomenon which

seems omitted from the explanation is that of the depression of fresh-water strata beneath the ocean, as for instance of coal measures of various ages. This however might arise from such deposits being depressed below the level of the sea, though not under the sea, through being loaded with fresh-water sediment, as is known from borings to be going on at present in great deltas and river valleys; and then, the process being checked, the sea might cut away the upper part of the sub-aerial deposit, and a marine stratum might take its place. It is possible that the fresh-water deposits becoming scanty, and the district arid, faulting might be induced through contraction of the beds before the sea had time to advance, and thus we should find the fresh-water beds much broken up before the marine were laid down over them.

One chief determining cause of the movements of the crust, as we have attempted to explain them, is the transference of sediment. Wherever that goes on, movements of the crust may be expected to take place. And although not altogether confined to these regions, it is obvious that it is in continental areas, and along their shores that these processes are the more energetic. It is therefore conceivable that the medial regions of the great oceans may have been comparatively free from disturbance from this cause at all times. Nevertheless the growth of coral, the deposits from icebergs, and the sinking of the exuviæ of marine organisms and of pumiceous fragments, are causes which may contribute to the unequal deposition of sediment, over such areas as these, and we would not therefore relegate even them to perpetual exemption from this source of disturbance.

It is certain that most of the existing greater mountain chains are comparatively modern; for it is possible to assign dates, geologically not remote, to the movements to which they owe their exceptional altitude. At the same time their structure usually betrays that the original effort which raised them was not the final one, for they have been subjected to repeated compression. This comparatively modern character of the greater chains leads us to believe that the gradual degradation and obliteration of a mountain range is a law of nature, in

accordance with which the older chains have been levelled down and disappeared. The corrugated rocks which may have formed parts of them are still to be discovered as fundamental gneiss for instance, showing by their chemical and mechanical condition that they have been subjected at some former time to a high temperature and to great compression. They are now frequently covered up by later deposits.

The rising upwards of an elevated region by floatation concurrently with its degradation, explains some well known facts, among which are the following.

When the original contours of the strata are restored, which must have gone to form a disturbed tract, the enormous amount of subsequent denudation appears at first sight incredible. This is extremely well shown in Prof. Ramsay's restored contours along some sections in South Wales[1], where the heights and distances are laid down upon the same scale, a method which enables us at once to appreciate the immense amount of material that has been removed. This is not a singular case. It is a universal phenomenon[2]. Now there is much less difficulty in conceiving strata to have been slowly lifted up from below concurrently with their degradation, than in supposing that the dry lands of old time were thousands of feet high where now we measure them by hundreds.

We have also the correlative fact of the excessive amount of sediment which an elevated tract has been found capable of yielding, and yet it remains an elevated tract still. Series of strata, five, six, or seven miles thick, and of great superficial extent, can be more easily thus accounted for, than by supposing the area from which they have been derived to have been five, six, or seven miles higher than it stands at present, even if it be now reduced below the sea level.

The doctrine that areas must sink when loaded, and rise when relieved of a load, may perhaps explain other known phenomena. It is not impossible that the raised shell beds of Scandinavia may be partly accounted for by the country having

[1] "Memoirs of the Geological Survey of Great Britain," Vol. I. 1846, pl. IV.

[2] See Powell's exploration of the Colorado River, p. 153. Washington, 1875. Also Geikie's Geological sketches, p. 208, 1882.

been depressed owing to its being formerly loaded with heavy icefields, and that its gradual subsequent rise may have been caused by the ice having been melted off. These beaches are found up to an altitude of about 700 feet[1]. Putting three feet of ice as equivalent to one of rock, a liberal estimate, 2310 feet additional of ice would, upon the suppositions we have made respecting the relative densities of the crust and substratum, effect this amount of depression[2]. This would be less than the average thickness of the icebergs seen from the Challenger in the Antarctic ocean[3].

Similar movements have occurred, and are now going on in Greenland. Raised beaches are found up to 326 feet above the sea. But the land is now slowly subsiding at the rate of about from 6 to 8 feet per century. This may possibly be accounted for by the snowfall being at present greater than is carried off by the glaciers and by evaporation[4].

Vertical movements of this kind, which will result from the laws of hydrostatic equilibrium simply, although they may for a long time continue to lift up the rocks of a given area from

[1] See Nordenskiold "On the gradual rising of the land in Sweden." "Nature," Vol. XXXIX. p. 488, 1889. Also Lyell's "Elements of Geology," Vol. I. p. 133, 10th edn. 1872. It may, however, be objected to this idea that several of the species in the shell beds are of a Mediterranean type. Croll's "Climate and Time," p. 253.

[2] Let x be the height of the surface above the mean upper level when there is no ice, y the depth of the root under the same circumstances; x', y', the like quantities when there is a thickness of z ice, supposing the depth of the ice to be represented by a corresponding thickness of rock.

Then we have the four equations,

$$y = 10x,$$
$$y' = 10x',$$
$$x' = x - 700 + \tfrac{1}{3}z, \text{ (see text)}$$
$$x' + y' = x + y + \tfrac{1}{3}z.$$

Whence $$z = 2310 \text{ feet.}$$

[3] Sir Wyville Thomson, "On the conditions of the Antarctic," "Nature," Vol. XV. p. 105, states that these stand about 200 feet high above water. Calculating from a specific gravity of 0·92 this gives 2,500 feet for the entire thickness.

[4] M. Johnstrup, "On the Interior of Greenland," reviewed in "Nature," Vol. XXI. p. 344, 1880. The above passage stands as in the first edition and was published before the appearance of Mr J. S. Gardner's article in the "Geol. Mag." Dec. 2, Vol. VIII. p. 241, where somewhat similar views are propounded.

below, can yet never restore them to their pristine height. Degradation must always make more rapid progress than elevation. But the two are necessarily correlatives of each other. The degradation of the tract causes more rock to rise up from below to be in its turn degraded. Such slow and continuous upward movement explains how rivers can cut across the dip of hills. It explains the drainage of such areas as the Weald, and of valleys of elevation on even a grander scale. It explains perhaps even the drainage of the great plateau of the Colorado region. It also explains the fact that we find what once were probably river gravels perched on the highest hills of a district. But the movements arising from the action of the internal liquid are of a different character. Their effect may be either to elevate or to depress a tract directly by an actual addition to, or diminution from, the volume of the matter underlying the crust, as the play of the currents increases or diminishes in intensity; or else, by thrusting it towards some particular region, they may compress it, thereby raising a range of mountains consisting of a subaerial ridge and a downward protuberance dipping into the subjacent liquid. The internal heat of the earth is the ultimate cause of these movements. The contraction of the interior from cooling may possibly contribute somewhat to compression, and the extravasation of ejecta, both solid and gaseous, from volcanos, by causing a diminution of the nucleus, may have aided in producing corrugation of the crust. The injection of lava into fissures may also have assisted. But it appears to us now, that the friction of currents upon the under side of the crust, has very probably conduced more largely than any other cause to the grand final result.

—E. L. S. Wrampelmeier

CHAPTER XXIV.

GEOGRAPHICAL DISTRIBUTION OF VOLCANOS.

The linear arrangement of Volcanos accords with the doctrine of a thin crust and liquid substratum—Volcanic bands related to the boundaries of continents —Distinction between coast-line and oceanic volcanos—Oceanic islands—Platforms on which oceanic islands stand—their possible origin—Great volcanic band of the Pacific coast—It follows nearly a great circle of the sphere—divides the land from the water-hemisphere and perhaps originated from some cosmical cause—Is not equally active at every part—Suggested cause of local activity—Other possible causes of the same.

THE Geographical distribution of volcanos presents perhaps fewer difficulties upon the supposition of a thin crust and a liquid substratum, than upon any other that can be made, regarding the constitution of the outer parts of the globe. The linear arrangement of the greater number of the vents, points to their situation along systems of fissures, and represents on a grand scale the same phenomenon, which occurs, when subsidiary cones of eruption are established upon fissures radiating from a central volcano. Why these fissures, extending for thousands of miles, should range in certain directions rather than in others, requires explanation. The causes which contribute to determine the direction of any fissure are very complicated, and in the case before us generally unknown; and where there were active vents in more ancient times there are often none now. The post-eocene volcanic bands are no doubt nearly related to the recent ones, but some of those of previous periods have probably little connection with any now existing.

The great volcanic bands of the present day have an obvious relation to the boundaries, which separate the oceans from the continents. They skirt the coast-lines, either upon the edge of a continent, as along Western America, or else they occupy chains of islands parallel to the continental shore, as is the case along the Eastern coast of Asia.

Besides these coast-line vents, others are found in the open ocean. These also appear to hold still a certain relation, though an altogether different one, to the coast-lines. Their position is medial. Thus in the Atlantic they occupy a medial line, rudely parallel to the opposite coasts[1]; and in the Pacific, the boundary of which is somewhat circular, they are always active in a central patch in the Hawaian group.

It also appears that an important distinction may be drawn between the modes of occurrence of coast-line and of oceanic volcanos. The cones raised by the former in some instances, no doubt, emulate in height, if they do not overtop, the crests of the ranges upon which they are parasitic. But they can by no means be considered as constituting a largely integral part of any mountain chain. If then such a chain were to be submerged, until only the tops of the highest mountains were left uncovered, the resulting islands would not as a rule consist of volcanic products, but, on the other hand, they would chiefly present crystalline or schistose rocks. Now the case is exactly the opposite with oceanic islands. They are all volcanic. Darwin, in his work on "Coral Reefs," tells us that "the geological nature of the islands which are encircled by barrier reefs varies;

[1] "The soundings of the Talisman in this region [of the Sargassum sea] show in a general way that, starting from the Cape Verde Islands, the marine bed falls regularly as far as about the 25th parallel, where it attains a depth of 6267 metres [3·894 miles]. Then it gradually rises towards the Azores and the 35th parallel, where it is about 3000 metres. These curves are far from agreeing with the curves indicated on the most recent bathymetric charts. The bed of the Sargassum sea seems formed of a thick layer of a very fine mud of a pumice nature, covering fragments of pumice and volcanic rocks. Here there would appear to stretch, at over three miles from the surface of the ocean, a vast volcanic chain, parallel with the African sea shore, and of which the Cape Verde Islands, the Canaries, Madeira, and the Azores, are the only parts not submerged." Preliminary report on the Talisman expedition to the Atlantic Ocean by M. Alphonse Milne-Edwards. "Nature," vol, 29, p. 198, 1883.

in most cases it is of ancient volcanic origin; owing apparently to the fact that islands of this nature are the most frequent within all great seas; some, however, are of madreporitic limestone, and others of primary formations, of which latter kind New Caledonia offers the best example[1]." So also are some of the Comoro Islands, and the Seychelles[2]. Now madreporitic rock, having probably grown upon a volcanic basis, need form no exception to the general law. And a glance at the map will show that neither New Caledonia, nor the Comoro and Seychelles Islands, are properly speaking Oceanic; the former belonging to the submerged ridge, which connects New Zealand with Australia and South-Eastern Asia, and the latter being a continuation of the axis of the great Island of Madagascar. The oceanic islands of the Atlantic are likewise volcanic. "There has not been found in the abysmal areas any land made up of gneisses, schists, sandstones, or compact limestones; nor have fragments of these sedimentary formations been found in the erupted rocks of the volcanic islands, though they are frequent in the volcanic eruptions of the continental areas[3]". There is then it seems the important distinction to be drawn between coast-line and oceanic volcanos, that the former are connected with axes of elevation, and the latter not so.

At the same time it is known that the volcanic oceanic islands rise from platforms elevated above the general level of the ocean floor. Still, if these were formed by compression, we ought to find some of the islands presenting exposures of the inclined strata of their higher ridges, which is not the case. Of what then do these platforms consist? It is possible that they may be partly accumulations of the ejectamenta of the volcanos themselves. With the fact before us that 665 feet of volcanic ash and tufa were pierced in sinking an Artesian well at Naples[4], we may well believe that the enormous volcanos, whose ruins now remain in the shape of Oceanic islands, may have laid down an immense thickness of deposits around their bases.

[1] "On the Structure and Distribution of Coral reefs." Second Ed. 1874, p. 62.

[2] *Ibid.* p. 69.

[3] Murray's lecture. Brit. Assoc. 1885. "Nature," vol. 22, p. 582.

[4] "Comptes Rendus," tome XLVIII. p. 994. 1859.

If this be a true account of the condition of these areas, the agency required for their production is one of fissuring without resulting compression. In the preceding Chapter we have explained how currents in the liquid substratum, ascending beneath the oceanic areas and flowing thence towards the continental areas, might be expected to have this effect of producing fissures, but being the consequence of tension of the crust these would probably not be accompanied by compression. Yet if the injection of lava into these fissures should have the effect of producing some amount of compression, it is still probable, seeing that the suboceanic crust is not much less dense than the substratum, even if there were compression, the resulting elevation would be comparatively small, because much more of the compressed matter would go to form the roots, than would be the case with a lighter crust.

It has been already remarked[1] that there is a distinction to be observed between an erupted cone and a mountain produced by compression. The process of formation of the latter causes a downward protuberance, which assists by floatation to support the weight. But no such protuberance accompanies the piling up of a volcanic cone. Consequently, where that is formed distant from any mountain chain, the tendency to rupture, and sink through the crust, will be uncompensated. In the oceanic areas the downward pressure of the cone will of course be lessened by the weight of the water displaced by it; but on the other hand the difference of specific gravity of the crust and the substratum being, as we believe, less than in continental areas[2], the support furnished from below will be less. The result will be that an oceanic volcanic area may have a tendency to perpetuate its own existence by fissuring the crust around its margin, and along the fissures so formed fresh eruptions might be expected to break forth. The elliptical form of many coralline archipelagos may perhaps lend countenance to such a supposition, for a train of volcanos, established originally upon a single line of fissure, would develop a figure of that form during the process we have suggested.

Reverting to the trains of volcanos which range parallel to

[1] p. 251. [2] p. 246.

the coasts of the great continents, we observe that, along the boundary of the Pacific Ocean, where near the American shore deep water is found not far from land, there the volcanos stand on the edge of the continent. But where, as in the Aleutian Islands, Japan, and the more southern parts of the western area of the Pacific, no great coast-range of mountains exists, and deep water is not found near the continent, there is found a great fall in the bed of the ocean on the outer, or eastern, side of the Islands which carry the volcanos. Here then, if the water were removed, the same fact would be apparent, that the volcanos occupy an elevated tract, which borders the great continent. Such an arrangement of the vents, regarded as the indication of systems of fissures below, of which probably a few only reach the surface, is in accordance with our theory, that the same ultimate cause underlies the phenomena both of compression and of volcanic activity.

Richthofen has made the remark that "active volcanos have been found to be particularly numerous in those regions where the narrow terminations of two continents verge towards connection, as in the case of Central America, between Alasca and Kamtschatka, and between Australia and Farther India[1]."

The great Pacific train of volcanos divides the world very nearly into two halves, as may be seen by placing an ordinary globe so that the wooden horizon passes over the Isthmus of Panama, the southern extremity of Kamtschatka, and the straits of Sunda. Near the latter place, however, the train becomes more complicated; for it takes a turn to the east at the Philippines, and is joined by that other train which passes through Java, New Guinea being at the place of junction, and the great Island of Borneo occupying the angle. Afterwards the combined train turns round towards the south, till it reaches New Zealand, following a course nearly parallel to the eastern coast of Australia.

If we look at the globe thus placed, we observe that we have nearly all the land on one side of this great train, and that the other hemisphere, except Australia, is occupied by water. It appears then that, with this exception, the said volcanic

[1] "Natural System of volcanic rocks," p. 79.

band defines the elevated part of the surface. But the exception proves the rule. For, when it nears Australia, the direction of the main band is diverted by its junction with the second, and within the great curve which their united course follows, this insular continent is embraced.

Regarding the great Pacific volcanic band under this aspect, it is seen to follow nearly a great circle of the sphere for more than half its circumference, when it is disturbed by encountering a second, which diverts it from its direct course. It can hardly be that any cause, less than one of a very general character, affecting the spheroid as a whole, can have produced this unique separation of its surface into land and water, accompanied by a continuous band of fissured crust round their junction. We are disposed to attribute its origination to some cosmical cause.

But this great band is not now equally active at every part. In seeking the conditions which produce those disturbances of the crust that determine the regions of present activity, by admitting at intervals of time the elastic fluids from below, we may look to changes in the distributions of load upon the surface, as has been more particularly discussed in the thirteenth Chapter. It has been there shown, that the less steeply inclined side of the elevated range will be that on which the principal sedimentation will go on, and, acting as it were on the longer arm of a lever, whose fulcrum is at the centre of gravity of the tract, it will tend to rupture the crust on the margin of the steeper side of the ridge, at the same time causing a certain hang of the tract towards the region of deposit, which will open rather than nip the crust at the place of fracture. We may possibly trace this connection between the situation of the active regions of the Western American volcanos, and the great rivers of that continent, which deposit their sediment on the eastern side. For instance, in Central America, we find a comparatively narrow volcanic tract, the western side of which is mountainous, while on the eastern the Mississippi deposits vast quantities of sediment. The region of its deposit is known to be a sinking area, and must tend to give a tilting movement to the tract so disturbed, raising and fissuring it along its western

border, near which its centre of gravity lies. The very same sedimentation may also be the cause of disturbance, where the West Indian volcanos occur.

The wide plain of the Amazon is also an area of great sedimentation, and may have an effect, similar to that just described, upon the coast line adjoining the Andes of Quito. In like manner the deposits from the Rio de la Plata may have a connection with the volcanic series of Chili.

Accumulations of snow and ice would have a like effect, and the subsidence of Greenland and the volcanos of Iceland may be possibly due to the accumulation of snow upon the former. These are however speculations which are merely put forward as suggestions.

There are other causes which, if the crust be no thicker than we have concluded it to be, may not be without effect upon its equilibrium; such are the action of ocean currents and winds. Slight as is the momentum of a mass of water, moving with the velocity of an ocean current, as compared with the mass of crust upon which it may act, nevertheless its effect cannot be nil. And the same may be said of winds, which, on the average of the year, have definite directions over a given tract of continent, and their friction upon the surface cannot be quite inappreciable. These two causes cooperating, may have had some share in modifying the outline and the surface contour of the land masses of the globe.

CHAPTER XXV.

A SPECULATION ON THE ORIGIN OF OCEAN BASINS.

The arrangement of land and water in hemispheres requires explanation—which must be in accordance with the conclusions already arrived at—Prof. Darwin's theory of the genesis of the moon—Is the Pacific Ocean the scar left by her removal?—The size of the cavity estimated—The density of the moon—How the cavity would be filled up—Change of position of the axis of rotation within the earth—Origination of continents—An illustration—Difficulties involved in Darwin's theory—Conclusion.

THE Author wishes it to be understood that he puts forward the suggestion contained in this chapter merely as a speculation, which he considers to rest on a far less firm foundation than the conclusions arrived at in the previous chapters. But as this speculation is closely allied to the subject in hand, and has been already published since the first edition appeared[1], it might be thought he had abandoned it, if no reference was made to it now.

In the preceding chapter attention was called to the remarkable circumstance that one hemisphere of the globe is almost entirely covered by an ocean, the Pacific, while the other consists chiefly of land, although divided into two principal continental groups by the Atlantic. The question arises, whether it is possible to account for this arrangement in conformity with the conclusions we have already arrived at, which are, that the terrestrial crust of which the continents is composed is less dense than that which underlies the oceans; that the continental crust has suffered compression, whereas

[1] "Nature," vol. xxv., p. 243, 1882.

that underlying the depths of the oceans has not; that the mean figure of the earth, as defined by the sea level, is a spheroid, so that the ocean basins are real depressions below the mean surface; and lastly, that the great oceanic and the continental areas have never exchanged places.

Professor Darwin has published an investigation upon "The Precession of a viscous Spheroid and the Remote History of the Earth[1]" which may perhaps throw light upon these facts. He thinks it probable that the Earth and the Moon at one time constituted a single mass, and that at the time when this mass was rotating at the rate of about one revolution in five hours the whole separated into two portions, the smaller of which went to form the moon: and that the moon then began to recede from the earth, until now after the lapse of fifty millions of years or more it is at its present distance. The ellipticity of the mass when rotating at the above-named speed would be about 1/12th. [This would make the spheroid at that time much less compressed than an ordinary orange.] He does not think it probable that this amount of ellipticity would cause the spheroid to break up simply from the centrifugal effect of the rotation; but he suggests, judging from the calculated period of a gravitational oscillation of a fluid spheroid of uniform density equal to the mean of the earth, viz. 1 hour and 74 minutes, that the period of the free oscillation of a spheroid, "consisting of a denser nucleus and a rarer surface" but of the same mean density as the earth, might coincide with the period of the bodily solar tide at that time (that is, the periods of the free oscillations and of the forced oscillations would be the same). "It seems to be quite possible that two complete gravitational oscillations of the earth in its primitive state might occupy four or five hours." "Accordingly the solar tides would be of enormous height." He then adds: "Does it not then seem possible that if the rotation were fast enough to bring the spheroid into anything near the unstable condition, then the large solar tides might rupture the body into two or more parts? In this case one would conjecture that it would not be a ring that would detach itself."

[1] "Phil. Trans. Roy. Soc.," Part II., 1879.

According to Prof. Darwin's theory, the act of fissiparturition by which the moon was thrown off must have been sudden. One of the two solar tidal protuberances broke away from the earth to inchoate a separate existence. A great but comparatively shallow hole must have been formed in the earth, whose centre would have been near the equator. Professor Ball in a popular lecture upon the subject[1] says: "Not for long would that fragment retain an irregular form; the mutual attraction of the particles would draw the mass together. By the same gentle ministrations the wound on the earth would soon be healed. In the lapse of time the earth would become as whole as ever, and at last it would not retain even a scar to testify to the mighty catastrophe[2]."

On the other hand, is it not possible that the basin of the Pacific Ocean may be that "scar?"

The density of the moon is 0·56654 times that of the earth. Putting the mean density of the earth at 5·5 this makes the density of the moon 3·1. The mass of the moon is 0·011364 of the mass of the earth. Calling the density of granite 2·68 it would accordingly require a layer about 31 miles thick of the density of granite to be taken off the primitive mass to make a body of the mass of the moon: but if the mean density of the matter removed was the same as that of the moon a thinner layer would suffice. And, if we reduce the area of the skin removed to the size of the oceans, it would need to be $31 \times 197/146$, or about 41 miles deep. Hence a uniform layer rather less than 41 miles deep taken off the oceanic areas would be sufficient to make the moon; and it might probably have furnished a mass of the proper density.

Of course the layer removed would not have been of uniform thickness. But the above estimate gives an idea of the size of the cavity that would have been produced. What then would happen to the earth? This would depend upon whether the surface had already become solid: and considering the rapidity with which the surface of lava solidifies, and that the hypothesis

[1] "A Glimpse through the Corridors of Time," "Nature," vol. xxv., pp. 79, 103. Reprinted, Macmillan & Co. 1882.

[2] "Nature," vol. xxv., p. 103. Reprint, p. 22.

requires the solar tides to have been for some time in action, it seems quite possible that there may have already existed a thin solid crust. If this was the case, it seems that the hole would have been filled up in the central parts by the rise of denser liquid from below, and have been contracted at the edges by the encroachment of the broken crust floating on the disturbed liquid, so that, when the raw surface again solidified, there would have been a fresh crust of greater density formed out of the heavy substratum over the middle area where the hollow had been left, and also in the channels between the fragment which had floated towards it; while it would be these portions of the original granitic crust, thus broken up into great and small fragments, out of which the existing continents and continental islands would have been in the course of ages evolved.

The sudden rupture of so considerable a fragment from the rotating spheroid would alter its mass, form, and moment of momentum. It appears then that its axis of rotation would have taken a new position within the mass; and this would account for the fact, that the approximate pole of the oceanic area is not at present in the equator, as must have been the case at the time of rupture.

We have suggested that the original crust would break up into larger and smaller fragments, and float towards the cavity. This would explain a certain rude parallelism, which exists between opposite coast-lines. Such is traceable between the western coast of America and the eastern coast of the Old World. The great islands of Sumatra, Borneo, New Guinea, Australia, and New Zealand, may have slowly floated from the area now occupied by the Indian Ocean to near the locality whence the moon finally broke away, towards which they seem to form a kind of trail of fragments.

The curvature of the remaining crust would be somewhat too small to fit the diminished sphere. This would involve a certain amount of crumpling to enable the fragments to adapt themselves to the increased curvature of the surface.

The hypothesis would explain the origination of rising convection currents beneath the medial areas of the oceans,

and these currents in the course of ages compressing the continental crust, aided by marine erosion of the coasts and other causes familiar to the geologist, would have rendered the adaptation of opposite coast-lines somewhat obscure though still traceable.

The accompanying plate is given to exemplify how when a continuous crust has been broken up, the fragments contracting upon themselves show this rude adaptation of the opposed edges. The traces of the paths, along which the contraction has taken place, are even better seen in the photograph than in the original. The wrinkles which correspond in some degree to mountain ranges are noticeable. The photograph[1] is taken from a portion of a badly painted window shutter, the wood of which was damp when the first coat of paint was laid on, and that again not yet hard when the second coat was added.

There are no doubt difficulties in the way of accepting Professor Darwin's theory of the genesis of the moon. One of these appears to be the commonly asserted absence of both air and water from the moon. It is not easy to understand how the superficial parts of the earth could have broken away without both air and water accompanying them[2]. Still further, it seems that the gravitational force in the direction of the earth must have so much exceeded the centrifugal force, that the moon could not have got clear away, unless it was shot off by some means to a greater distance from the earth's centre than tidal action is likely to have carried the protuberance from which it was detached[3]. Possibly the presence of dissolved

[1] By Harold Hurrell, Esq., with the permission of the Rev. G. C. Clements, Haslingfield Vicarage.

[2] The Rev. F. F. Grensted thinks that the airless and waterless condition of the moon may be accounted for by the oxydation of her substance in the lapse of ages. "Trans. of the Liverpool Geol. Soc." Nov. 8, 1888. If however our view of the constitution of the substratum is correct it would have been already oxydised when detached. See p. 150.

[3] Prof. Darwin writes in a reply to criticisms by Mr Jas. Nolan on his papers—"When a small satellite revolves about a planet with a certain proximity, the sum of the centrifugal and tidal forces may be such as to overbalance the gravitation towards the centre of the satellite. When this is the case, the satellite cannot exist as a single mass.. It may be admitted that the moon could not exist as a single mass with its surface in contact with the earth." "Nature,"

gas may have had some such explosive action. But the question is as yet involved in much obscurity; while any speculation built upon this theory of the genesis of the moon necessarily shares the weakness of the foundation. Still it must be admitted that, if this theory of the genesis of the moon seems to give a fairly rational explanation of the origin of ocean basins, it will itself receive support from that circumstance.

At any rate the accompanying photograph is interesting, representing, as in a small model, a generalised idea of the formation of continents and large islands, by compression of a crust, owing to a stress thrusting it away from the intervening *quasi* oceanic areas.

In bringing this volume to a conclusion, the reader is reminded that there are several phenomena which have not been noticed which appear to require for their production causes hitherto unexplained, which must be looked for possibly outside the globe itself. Such are climatal changes. Such is also the remarkable fact that the grand efforts of elevatory action appear to have been paroxysmal, and after slumbering for long ages, to have been more intense at certain periods; the last of which was subsequent to the Eocene. These questions are left for a rising generation of physicists and astronomers, who it is hoped may be encouraged by the present attempt to think the ascertained facts of Geology a worthy field for the application of exact scientific methods.

Feb. 18, 1886. He appears to admit that the moon must at first have existed as a "flock of meteorites" which, he says, would none the less be capable of raising tides in the earth, and would themselves recede from it, and at some period become conglomerated into a single mass.

In his subsequent paper on the "Figures of equilibrium of rotating masses of Fluid," "Phil. Trans." vol. 178, 1887, p. 416, he says, "Thus as far as this investigation goes, it appears that, when the fluid moon is on the point of breaking up from stress of tidal and centrifugal forces the distance between the centres of the moon and earth is 6500 miles, and the shortest distance between the surfaces is 380 miles. This result however, from the nature of the approximation, must be an under-estimate of the distances." He adds that this section was suggested by Mr Nolan's pamphlet.

The meaning seems to be that when the flock of meteorites had got to such a distance from the earth that, when conglomerated, the surfaces of the earth and of the conglomerated mass were 380 miles apart, such conglomeration could take place—but not before that distance was reached.

SUMMARY.

[The Roman numerals refer to the Chapters.]

I. *On underground temperature*—II. *Internal densities and pressures*—III. *Condition of the interior*—IV. *Change of density on solidification*—V. *A liquid substratum dissolving gas according to Henry's law may account for the absence of tides at the exterior surface of the crust*—VI. *A thin crust implies an energetic substratum*—VII. *Lateral stresses and resulting inequalities of surface*—VIII. *Results of the cooling of the earth, supposing it solid throughout*—IX. *Theories to account for inequalities of surface*—X. *Hypothesis of solidity fails*—XI. *Liquid substratum*—XII. *Crust not flexible*—XIII. *Disturbed tract*—XIV. *The revelations of the plumb-line*—XV. *The revelations of the pendulum*—XVI. *The revelations of the thermometer*—XVII. *The suboceanic crust*—XVIII. *Island attraction*—XIX. *Amount of compression*—XX. *Disturbance of rocks*—XXI. *Volcanic dykes*—XXII. *The volcano in eruption*—XXIII. *Geological movements explained*—XXIV. *Geographical distribution of volcanos*—XXV. *A speculation on the origin of ocean basins.*

I. We have commenced our discussion of the wide subject of the Physics of the Earth's Crust with underground temperature, because the distribution of heat in the interior of the earth is one of the cardinal conditions upon which all physical questions connected with it depend. We have pointed out that, having regard to such depths as artificial excavations reach, the law of increase is on the whole an equable one, amounting on an average to about one degree Fahrenheit for every 51 feet of descent, if it be not even slightly more rapid. It has been asserted that the rate has been found to decrease towards the bottom of some deep boreholes, and it has been even maintained that at a depth of about 5000 feet the increase of temperature would come to an end. But we have shown reasons for

believing, that the observations, in which a diminution in the rate has been said to have been noticed, have really been vitiated by the disturbance of the column of water in the borehole by convection currents, and that the supposed cessation of the increase at about 5000 feet has arisen from a delusive method of computing the general result. Nevertheless it is unquestionable that there must be a diminution, and that this equable law of increase of temperature, though sensibly true near the surface of the earth, cannot extend to all depths; for, if it did, we should have, at the depth of from 200 to 400 miles, temperatures which would equal those, which have been on good authority attributed to the sun itself. The equable law of increase, however, may be so far depended on, as to lead us to expect at most localities a temperature at which the rocks would melt at a depth of certainly less than 30 miles.

II. We are next led to speculate about the condition of the earth's interior. And first we enquire what is the law of density? The mean density of the surface rocks can be fairly estimated by actual observation in more ways than one, and is found to lie between 2·56 and 2·75 times that of water. The mean density of the whole earth has been determined by experiments with the plumbline, and torsion balance, and may be taken to be about 5·5. There is little doubt that the interior of the earth is in a sense stratified, and consists of layers, or *couches de niveau*, each of equable density, and that the density is greater towards the centre, and diminishes towards the surface. But it is not known for certain what the actual law of density is; that is to say, we do not know how great the density is at any given depth, nor how thick any stratum is, which may be supposed to have a particular density. All that we really know is that the heavier layers are deeper down than the lighter, and that the forms of them are spheroidal, becoming more and more flattened about their poles as they approach the outer surface.

Now these are the arrangements and forms which the strata of a liquid spheroid would assume under the action of rotation and gravitation. It has been consequently concluded that the earth was once wholly melted.

The mean density of the whole earth is known, and also that of surface rock. But the actual densities of the successive layers, and their corresponding forms and thicknesses, are not known. There must be certain relations among these attributes in order to satisfy the ascertained facts regarding the earth's shape, and its mean and surface densities. But these requirements may be satisfied in more ways than one. Laplace suggested a law of density, suitable for purposes of calculation, which satisfies these requirements very closely; and that law has been generally assumed, as representing the true state of the case, in investigations on the figure of the earth. It makes no assumption as to the actual cause of the variation of density with the depth, although it is assumed to be such as might be caused by a certain relation between density and pressure.

Prof. Darwin has lately proposed a different law which in some respects gives equally good results with Laplace's. But we have given reasons for concluding that Laplace's law, which has been generally made use of in investigations on the figure of the earth, gives the better representation of the facts.

It is likely that the increase in density towards the centre of the earth arises from the heavier materials gravitating thither. Waltershausen has formed a theory on the ground that the earth is a hot globe, of which a considerable portion is fluid, an unknown amount of the central parts being perhaps rendered solid by pressure. The downward increment of density is expressed by the chemical increment of the heavy bases; and the liquid region directly under the crust consists first of a felspathic and acid magma which passes downwards by successive replacement of bases into an augitic and finally into a magnetitic magma. This theory appears to have a great deal to recommend it. Waltershausen has given what he thinks the probable densities and pressures at different depths, but the formula by which he has calculated them appears to have been arbitrarily assumed. He makes the density at the centre of the earth 9·59, or about that of silver, and the pressure there 2,500,000 atmospheres. If however we use Laplace's law of density, the density at the centre will be nearly 11 and the

pressure at the centre comes out considerably greater, reaching that of 3,000,000 atmospheres.

III. The present law of temperature being such that we may expect to find heat enough at the present day to melt the materials of the earth at the depth of less than 30 miles, and mathematical investigations, concerning the figure of the earth and its law of density, leading to the conclusion that it has been melted, two lines of argument meet in favour of fusion. The question then arises, whether it is at this day molten within, either wholly or partially, or has the molten condition passed away, so that it is now solid? There is however a *tertium quid.* Some have thought that it is what has been called 'potentially liquid,' that is, wholly solid in consequence of the pressure to which the internal strata are subjected, but ready to become liquid, on account of its high temperature, whenever the pressure is relieved. This is the great subject of controversy, towards the determination of which the present volume is partly intended as a contribution.

The increase of temperature below the outer crust, which we know to be solid, will be governed by the laws of convection of heat if the interior is liquid, but by the laws of conduction if it is solid.

The question whether the interior of the earth is at present solid or liquid, or partly solid and partly liquid, may, apart from geological considerations, be attacked in two ways. The first of these is by enquiring, what is the difference of effect that the moon and sun would have upon the motions of the earth in either case; and the other way is by considering the sequence of events, according to which a molten globe may have passed into its present state. For the first, there are two kinds of effect produced by the attraction of the moon and sun upon the earth; and these are Precession and the tides. Mr Hopkins, between 1839 and 1842, investigated the difference between the precessional effects in the cases of a solid and of a liquid interior on certain assumptions, and came to the conclusion that the crust of the earth could not be less than from 800 to 1000 miles thick. But the arguments of Mr Hopkins were, in 1876, abandoned by Sir William Thomson. Moreover Professor

Darwin has found that "The precession of a fluid spheroid is the same as that of a rigid one, which has an ellipticity equal to that due to the rotation of the spheroid."

With respect to the tides the case is different. Sir William Thomson says that unless the earth as a whole was extremely rigid, "the solid crust would yield so freely to the deforming influence of the sun and moon, that it would simply carry the water of the ocean up and down with it, and there would be no sensible tidal rise and fall of the water relatively to land." Subsequent investigations have however led mathematicians who have studied the subject to conclude that the semidiurnal tide, which is so obvious to every one, is not suitable to settle this question on account of the difficulties connected with the theoretical calculation of its amplitude: and the fortnightly tide is the one which is preferred for the purpose.

This tide so far as has been hitherto ascertained may be said to exist only on paper. That is to say calculation shows that the moon's action on the waters ought on the hypothesis of the earth's rigidity to raise the ocean along the equator by about four and a half inches once a fortnight, but it was not known whether it actually did so. Accordingly the attempt has been made to discriminate this tide among the oscillations to which the waters are liable. This investigation has been rendered possible by the long continued and numerous tidal observations which have been carried out under the direction of the Indian government. Nevertheless the result on the whole must be admitted to be somewhat inconclusive: for the fortnightly tide appears to be so masked by atmospheric disturbances, that, even if it does exist, it cannot be satisfactorily detected, and when we bear in mind the smallness which calculation assigns to it this is not surprising. At present therefore the argument for the great rigidity of the earth derived from the presumed existence of this ocean tide, and the consequent absence of a corresponding bodily tide in the earth, does not appear to be complete[1].

[1] What we have stated in the text seems to be a legitimate conclusion from the great irregularity in the tabulated values of the annual means given in the "Results of the Harmonic Analysis of Tidal Observations," by Major Baird and Prof. Darwin, "Proc. Roy. Soc." vol. xxxix., p. 135, 1886, this being the latest

IV. We next enquire what may have been the sequence of events according to which a molten globe has passed into its present state. Hopkins concluded that "the crust of the earth must be now, or have been at some antecedent epoch, in that state in which a solid exterior rests on an imperfectly fluid and incandescent mass beneath." But this conclusion has been controverted on the ground that cold rock must be more dense than molten rock, and that consequently a crust could not be supported upon a fused substratum, but would break up and sink into it. We have endeavoured to gather such records of experience as can test the question. This is like all other questions which relate to the physical properties of a substratum of fused rock, in that they are hardly capable of being decided in the laboratory, and observations at volcanos appear to be the more hopeful. Neither the one nor the other however appear to give very decisive results as to the relative densities of solid and molten rock. The great crater of Kilauea offers exceptional facilities for such enquiries, and on the whole it appears that the evidence there is in favour of lava being of less density when solid than when in fusion, and that the sinking of the lava at certain stages of the phenomena is due to its being sucked down by descending currents, rather than to an excess of density. But whatever may be the relative densities of solid and liquid lava of identical composition, it is obvious that solid rock of a more silicious character would float upon a substratum consisting of a heavy basic magma.

V. Let us accept the inference that no appreciable tides are raised in the body of the earth by the attraction of the moon, although it is somewhat doubtful whether this conclusion is capable of complete verification, owing as already stated, to the difficulties of observation and the smallness of the tide attempted to be discriminated. If however no such bodily tides are produced at all, it has been mathematically proved that the earth must be extremely rigid throughout, provided that it consists of incompressible materials.

publication on the subject. This irregularity is especially noticeable in the case of Karachi, for which port the results of observations extending over a period of fifteen years are given.

Are there then any means of judging of the nature of these internal materials? Volcanos furnish us with the information by bringing up specimens to the surface: and we find that their ejecta of fused rock are accompanied by large evolutions of steam and gas. This suggests the intimate association of water above its critical temperature, and therefore in a gaseous state, along with fused rock beneath the crust of the earth. Henry's law of the absorption of gases by liquids indicates the probable nature of this association. Accordingly we have calculated the changes in volume, which a magma so constituted would undergo, when subjected to varying pressures, and it appears that, under conditions which are by no means unlikely, a liquid substratum so constituted need not cause any tidal rise and fall of the crust at its surface under the varying pressures arising from the attraction of the moon upon the substratum. In this way the principal objection to the doctrine of a thin crust and liquid substratum can be met; although, as has been explained above, it does not seem certain that the objection in question is decidedly supported by observation.

If the hypothesis of the association of water in a gaseous condition with melted rock accounts for the two seemingly independent phenomena of the emission of great quantities of steam from volcanos, and also for the absence of bodily tides in the earth, the fact that it does so gives a decided support to the hypothesis, that the substratum is really constituted in that manner.

VI. Having formed the hypothesis that the earth became solid throughout in a comparatively short space of time, Sir W. Thomson adapted a mathematical formula to express the present temperature within it. Many geologists however are strongly disposed to dispute this hypothesis, and we have shown in the preceding chapter that the argument for entire solidity does not appear to have in reality the force which, from the high authority of its supporters, has been accorded to it. We have consequently adapted Sir W. Thomson's formula for the temperature within the crust to the case of a crust, which is still in the course of formation through progressive solidification by freezing out of a fused substratum. An element in the pro-

blem is necessarily the latent heat of rock. This has not been experimentally determined, but it is almost certainly less than that of water, which is the highest known. Assuming this to be so, the results at which we have arrived are sufficiently remarkable.

The first case that we have considered is that in which the substratum is supposed to be an inert liquid; so that the freezing goes on uninterruptedly from above downwards in a still medium. In this case we find that, if the crust is no more than 25 miles thick, the age of the world cannot be greater than 11 millions of years. Even if the crust is 50 miles thick, the age of the world cannot be greater than about 44 millions of years. But 25 miles is quite as great a thickness as the advocates of a thin crust would be disposed to grant. We see therefore that the age of the world, which under these circumstances would correspond to a thin crust, is much less than geology presupposes; and the presence of an inert and motionless substratum is thus rendered in the highest degree improbable. The conclusion is inevitable that, if the crust of the earth is about 25 miles thick more or less, the liquid stratum which underlies it cannot be inert. The crust cannot be in the course of formation out of a motionless liquid; but rather the bottom of the crust must be prevented from freezing, as fast as it would otherwise do, by heat being brought up by some means from below, so as to melt it off somewhat less rapidly than the freezing would otherwise progress. The only way in which this can be effected is by convection currents within the substratum. The case is analogous to that of a sheet of ice forming upon a lake from the bottom of which warm springs arise. Every skater knows how thin and treacherous the ice is apt to be on such a lake.

Currents of this kind cannot take place in a medium which is not truly mobile, so that we are led to the conclusion that the substratum of the earth's crust is more than viscid. It is actually liquid. Here we find an explanation of the ever varying changes of level which affect the crust. For convection currents depend upon slight disturbing causes, and are apt to shift their places; and it can hardly be otherwise than that

internal displacements of mass must consequently occur, which would periodically modify locally the curvature and contour of the supernatant crust.

This view of the condition of the interior appears to gain support from a quarter altogether removed from the domain of geology, for after discussing the mysterious phenomena of magnetic variation the late Sir F. J. Evans concluded a lecture at the Royal Geographical Society with these remarkable words. "These are a few facts relating to secular changes going on in two magnetic elements within our own time; and what are the inferences to be drawn therefrom? They appear to me to lead to the conclusion that movements, certainly beyond our present conception, are going on in the interior of the earth; and that so far as the evidence presents itself, secular changes are due to these movements, and not to external causes. We are thus led back to Halley's conception of an internal nucleus or inner globe, itself a magnet, rotating within the outer magnetized shell of the earth[1]." This is, to say the least, in very remarkable accordance with the conclusion that a liquid stratum, not motionless, underlies the cooled crust of the earth.

The accretion of solid material at the underside of the crust being very gradual the rock as it solidifies there would probably assume a crystalline rather than a vitreous character.

VII. Our next enquiry relates to the compression which has again and again affected those parts of the earth's crust, which we have access to and can observe. Of the existence of such compression there is no doubt, but the cause of it is far less certain.

The well known fact has been generally explained by the supposition that the globe has contracted through secular cooling. It has been thought that, as the cooling proceeded, the interior shrank away from the crust, and the latter became wrinkled; and that by this means the crumpling and contortions of the rocks were produced. We have accordingly calculated what the lateral pressure would be, which would be available for crushing the strata of the earth's surface, supposing

[1] Lecture at the Royal Geographical Society, March 11, by Capt. F. J. Evans, C.B., F.R.S., Hydrographer to the Admiralty, "Nature," May 16, 1878.

that the interior were to shrink away from the crust, and to leave it unsupported. We find that it amounts to the enormous pressure of the weight of a column of rock of the surface density, of the same section as the stratum, and *two thousand* miles long, or about 830,200 tons upon the square foot. We need not doubt that this pressure would be competent to perform the work expected of it.

That the pressure thus produced would be abundantly sufficient for the purpose, is however no proof that the work has been accomplished in that way. It has been an assumption often repeated, but never proved. One task which we have proposed to ourselves is to examine this point. We admit that inequalities of the earth's surface have been caused by lateral compression, but we are not sure that this has arisen from the secular cooling. We ask whether that cause would have been sufficient in amount to give rise to chains of mountains of the magnitude of those which exist, and whether chains produced in that manner would be arranged upon the surface in the manner that they are.

To begin with it is necessary to seek some measure of the inequalities of the surface, as a preliminary step towards determining how they have been produced: and in the first instance we include the greater inequalities, which constitute the oceanic and continental areas. It must however be recollected that, while we have ocular proof that mountain chains have been formed by compression, it is a mere matter of inference that the elevation of continents above the ocean floor is due to the same cause.

Suppose then the earth to have contracted through cooling, and suppose (which is of course impossible) that the crust had shrunk down upon it without becoming either thickened or wrinkled by the process. The position which the surface would have occupied under this impossible supposition, gives us a definite level, at a definite distance from the centre of the earth, and this level we call the 'upper datum level.' Similarly, the position which the under side of the cooled crust would occupy, we call the 'lower datum level.' To these levels we refer the elevations, and depressions (if any), of the surface, caused by

the crumpling action; and we can express their amount by saying how deep a layer of material the elevations would form, if they were levelled down, and spread out upon our upper datum level. This will give the mean height of the elevations.

Now if we assume that the earth is solid, *i.e.* that there is no liquid substratum beneath the crust, it is clear that all the inequalities which compression could produce would be of the nature of elevations above the upper datum level. There could be no depressions. Moreover the bottom of the ocean basins would either coincide with this datum level, or else they would be the parts of the disturbed surface least raised above it.

This mode of measuring the lateral compression, viz. by the thickness of the layer that the resulting corrugations would form if they were levelled down, appears to be more satisfactory than the usual method of supposing the folds flattened out, because we can only guess at the lengths of the members of the folds, of which the upper parts alone are visible.

The strata have however been subjected in some places to an action the opposite of compression, and have been extended, as in the case of direct faulting. The amount of this effect can also be measured in a manner analogous to that adopted for compression, viz. by considering the mean depth of the depressions, which would be formed below a datum level, defined by the supposition that the strata had been perfectly extensible.

VIII. Having thus fixed upon the mean height of the elevations as a suitable measure of the compression by which they have been produced, we proceed to calculate what the amount of the elevations would be, if they had been formed by lateral compression out of an earth, which had solidified throughout almost simultaneously; because that is the hypothesis to which some leading physicists would restrict the geologist.

It is only recently that it has beeen noticed by Mr Reade and by Mr Davison, that a solid globe, cooling in the manner supposed, would contain at a comparatively small depth below the surface a level, or shell, where the rocks would be neither compressed nor extended. We term it the level of no strain.

Above this level the strata would be compressed, and below it they would be extended. It will therefore be solely out of the rocks which lie above this level that elevations caused by compression could be derived. Sir W. Thomson long ago established the law, which the temperature within the earth would follow, if it were a solid globe cooling by conduction. We are consequently able to say how much it would have cooled at any specified depth from the surface, since the whole became solid. If then we know the amount of contraction on cooling for such rocks as exist near the earth's surface, we shall have the means of calculating how great the contraction would have been; and consequently what amount of inequalities this cause could have produced. Again, Mallet has made a series of careful experiments on a large scale, on the contraction through cooling of silicious slags from a state of fusion, and his results are sufficiently applicable to our problem. These we have used, and calculated the amount of inequalities of the earth's surface, which would have been formed had they been due to a hot solid globe cooling by conduction, and we have found the amount to be extremely small: for the mean depth of the layer, which the elevations so formed would yield when levelled down, would be no greater, on an extravagant estimate of the temperature of solidification, than a little over six feet[1].

The thickness below the level of no strain, throughout which cooling would have been sensible, and the rocks consequently subject to contraction, would have been greater than that above it through which they would have been compressed. Here the strata would have been shortened horizontally, as well as vertically, so that there would have been a tendency to stretching. But owing to the enormous weight of the superincumbent strata,

[1] During a discussion in the Geological section of the British Assoc. 1887, upon the subject of the level of no strain, some of the speakers appeared to think that, since it was certain that rocks have been raised by compression from a depth greater than the greatest which could be assigned to this level, therefore it must be a mistake to suppose that such a level of no strain can have any existence. But it will be seen that this argument assumed, that the rocks have been elevated by compression owing to the cooling of a solid globe, whereas the very question in dispute is, whether that has been the sufficient operative cause of their elevation.

no vacuities could have been produced by this means; and there does not appear to be any reason why these effects should be localised, so as to cause subsidence of the surface in some areas exceeding that in others. It does not therefore seem possible to attribute the formation of ocean basins to this cause.

All the above considerations refer to the case of a cooling solid globe. If the crust reposes on a liquid substratum, the layers, successively added to it from beneath, will have been of the fitting size at the time of their addition, and the conditions of the problem of contraction would be greatly altered.

IX. Having examined the theory of a cooling solid globe, and calculated the mean height of the elevations which would result from contraction on that hypothesis, we next enquire what is the mean height of the elevations actually existing upon the earth's surface, measuring them above the lowest parts of the ocean bottom; and we find it to be 13,000 feet. The mean height of the land above the surface of the ocean alone is 1,000 feet. We conclude therefore that the "contraction theory", under that form which attributes the inequalities to a cooling solid globe, is wholly incapable of explaining the facts. This affords one argument against solidity.

It has however been objected that the earth may nevertheless be now solid throughout, and may have been so from primitive times, in spite of the inability of the contraction theory to account for the existing inequalities; because it is asserted that they may, consistently with solidity, be accounted for in another way, which has been suggested by Professor Darwin. We have given a description, at page 115, of his theory of primitive wrinkles, which it is unnecessary to repeat, and have pointed out that the wrinkles, which he supposes would have been so formed, can hardly be considered to conform in position and direction with the existing mountain ranges, and that whereas the theory would require the orographical systems on opposite sides of the equator to be alike, they are in fact essentially unlike. Still further, it may be replied, that although the existing mountain ranges were originally shaped out in very early times, nevertheless the movements which had given them their present loftiness, are in most cases by no means

primitive, but geologically speaking often quite recent; a circumstance which points to the permanence of some exciting cause, whereas the theory of primitive wrinkles proposes one, which must have long ago ceased to operate effectively.

X. In addition to the argument against solidity derived from the impossibility of accounting for the existing compression by the cooling of a solid globe, we proceed to point out that the radial contraction of a solid globe could not produce the depressions in which the oceans are contained, because if they were due to that cause, it could be only to the excess of radial contraction beneath oceanic areas over that beneath the continents to which they could be attributed. The entire radial contraction of a solid earth on an extravagant estimate we have shown to be only six miles, and on a more moderate estimate two miles. It is therefore obvious that difference of radial contraction could not account for depressions, which are of an average depth of between three and four miles.

Our next argument against the solidity of the earth is strictly geological, and probably on that account has been almost overlooked by physicists. It is deduced from crust movements, which extend to all regions of the surface and have been confined to no particular period of the world's history. These well-known geological phenomena, which prove the instability of the earth's surface, negative the hypothesis of solidity. Alternate elevation and depression have frequently affected the same areas. But especially the shifting of the crust towards a mountain-range, which is testified by the corrugation of the rocks of which it is formed, requires a more or less liquid substratum to admit of it. The sinking of areas such as deltas, and other regions of deposition, demands a like arrangement; and in short, it appears that the crust, in the form in which it exists, must be in a condition of approximate hydrostatic equilibrium, such that a considerable addition of load will cause any region to sink, or any considerable amount denuded off an area will cause it to rise.

Movements of this nature require a thin crust resting upon a yielding substratum, while the phenomena of underground temperature and volcanos show that the stratum is hot, and

probably holds gas in solution. But we have proved in the sixth chapter that the crust formed by cooling out of such a substratum cannot be thin, unless it is prevented from growing thick by the substratum constantly dissolving off, and as it were washing away, its underside; not so fast, but nearly as fast, as it solidifies. This, as has been already explained, shows that there must be convection currents in it, and that it must therefore be truly liquid.

XI. The hypothesis, that the substratum is composed of molten rock holding gas in solution, is little more than an extension of Scrope's theory, that there exists within and below volcanic vents a body of lava of unknown dimensions, permanently liquid, at an intense temperature, and continually traversed by an aeriform fluid. We have merely added that this gaseous fluid is in solution in accordance with Henry's law, and that the reservoir is coextensive with the entire substratum. Henry's law explains why, on account of the pressure keeping it dissolved, this gas would not have all escaped by ebullition before a crust could be formed upon it. But convection and diffusion from below would bring up fresh supplies of gas, so that the liquid at the bottom of the crust would in general be maintained saturated. Volcanic eruptions then follow from this substratum gaining access to the surface.

It is a well known fact, that all volcanic eruptions are in a greater or less degree accompanied by the emission of steam; but it is not agreed whether this water-substance forms an integral part of the substratum, or whether it becomes in some way subsequently mingled with the lava during, or just before, its eruption. The position of most volcanos near the sea, and the sea salts given off by the lava, have been held to favour the latter supposition. Water can be conceived to gain access to masses of heated rock only in two ways; by open passages, or by capillary absorption. We have, we think, shown that neither mode of access is possible. It remains that this water, and the elements of sea salts, must be original constituents of the magma. The question then is, how this water-substance comes to be there. To answer this it is necessary to go back to the cosmogony, and we suggest that, under the pressure due to the

water-substance, now condensed in the oceans, which would have, in a state of vapour or gas, formed the outer layers of the still incandescent earth, this vapour or gas would not have been necessarily superincumbent on an anhydrous globe, but that such rocky matters as were capable of holding some of it in solution would have done so; and we point out that the enormous amount of oxygen, present in the deeply seated no less than in the superficial rocks, proves that that element has had access to the bases during primitive times, when even those bases themselves were possibly in a gaseous state. If then oxygen was present, it is probable that hydrogen was so likewise, and that while, as cooling proceeded, the earthy bases took their share of the oxygen, so the hydrogen took up its share also. In this manner water-substance would have been a constituent of the magma, as well as what, from this point of view, we may call the other minerals.

XII. In considering the form and arrangement of the inequalities, which a thin crust might present, if it rested upon a liquid substratum in corrugations produced by lateral compression, the supposition may be made that the crust is flexible, that is, that it accommodates itself to its position by bending without breaking or becoming thickened anywhere, as does the skin which forms on boiled milk when it is cooling. Under these circumstances the liquid would rise into the anticlinals, and in geological diagrams this has been frequently represented to occur. We have therefore thought it desirable to calculate the form, which under such circumstances a crust would assume, and have shown that there is no reason to think, that it accords either in form or dimensions with the inequalities on the earth's surface. We consequently feel justified in dismissing this hypothesis.

If the crust does not maintain a uniform thickness, it must accommodate itself to compression by being crushed together, and thickened in places. The very important consequence follows, that elevations above the datum level will be accompanied by depressions beneath. The anticlinals will not be filled with liquid from below, but will be the upper portions of double bulges, which will dip into the liquid below, as well as

rise into the air above. From whatever cause compression might arise the result would be the same.

XIII. We next attempt to deal with the condition of the earth's crust just described. It will be observed that it is analogous to the case of a broken-up area of ice, refrozen and floating upon water. The thickened parts which stand higher above the general surface also project deeper into the liquid below. There will also be what we call an effective level belonging to the liquid, which is the level to which, if it was inert, it would rise in a hole carried through the crust. We have supposed the crust to have the specific gravity of granite, and the liquid substratum to have that of basalt. Hence their ratio is about 0·905, whilst the specific gravity of ice is 0·9176. These numbers are so nearly the same that the two cases are exceedingly analogous, and the downward protuberance of the crust, as compared with the elevations above the surface, will agree closely with the immersed part of an iceberg as compared with the part exposed.

The work done by a compressing force in raising a mountain range will be distributed between the work done against gravity in elevating the mountains, and depressing the immersed crust into the denser liquid below, and that of deforming the crust, producing distortion of the rocks and cleavage. Upon investigation we find that, as regards gravity, compression through a given horizontal space is more easily satisfied by raising a long tract through a small elevation, than by raising a short tract through a great elevation. The case is analogous to the support of a range of water by a lock gate. As regards the other department of work to be done by the compression, the disturbed length will be greater or less according as the crust is weaker or stronger. The substratum may also offer frictional resistance to the shearing of the crust over it, and part of the work would be expended in overcoming this. On that account the work left available for thickening the crust and raising its centre of gravity will diminish, as the distance from the place of application of the force increases, and the disturbed tract will be most lofty on the side from which the compressing stress comes.

When the crust is crushed together along a mountain-range, part of the mass will be sheared upwards and part downwards. If it was equally rigid from top to bottom, half of it would go up and half of it down, and the neutral zone, as we have termed it, will be at half the depth. But, if the lower parts be softened by heat, a greater thickness will be sheared downwards than upwards. If the softening were to follow a certain assumed law which would make it increase slowly at first but more rapidly at last, until it mingled with the liquid substratum, the neutral zone would be at the depth of one-third of the crust. This would be probably an excessive estimate. We accordingly have assumed a value between the two, and place the neutral zone at the depth of two-fifths of the whole thickness.

And here we arrive at a stage at which we make our first attempt at estimating the actual thickness of the crust. Granite has been formed in the presence of liquid water as proved by Mr Sorby. Water cannot remain a liquid at a higher temperature than 773° F., whatever pressure may be placed upon it. This temperature, at the rate of increase of 1° F. for from 51 to 60 feet, would be found at the depth of from 7 to $8\frac{1}{2}$ miles. Hence rock, which was once at the depth of from 7 to $8\frac{1}{2}$ miles, has been forced upwards; and therefore the neutral zone is deeper than that. But if 2/5 of the thickness were 7 to $8\frac{1}{2}$ miles, the whole thickness would be from 17 to 21 miles. Hence we conclude the whole thickness to be greater than this. Again, taking the temperature of melting slag at 3000° F. (an excessive estimate) such a temperature would be met with at the depth of from 28 to 30 miles, and we can hardly suppose the temperature of the substratum to be so high as that of melting slag. Hence we place, roughly, the thickness of the undisturbed crust as a first attempt between these estimates, at about 25 miles.

Now supposing a tract of the crust crushed together by lateral compression; and that about two-fifths of the thickness goes up, and three-fifths goes down. If it were to remain in this position, we should have the ratio of the part above the effective level of the liquid to the part below it as 2 to 3. This would we impossible if it floated; just as it would be

impossible that an iceberg should stand 200 feet above the water while only 300 feet were immersed. But the tract of crust does not exactly float; for it is held up to some extent by its attachment to the neighbouring crust. Nevertheless it cannot be held up long in what would be so constrained a position. It must then sag downwards; and the most thickened part would sink the most. Hence depressions would arise on both sides of the ridge, and the ocean, which covers the general surface, would be deeper than elsewhere along two channels parallel to, and at some little distance from, the ridge. But should the ridge be steeper on one side than on the other, as seems inevitable, the ocean would be deeper on the steeper side. This relative position of the depths of the ocean to mountain-chains is in accordance with nature.

Next suppose the ridge thus elevated to be denuded. The chief streams will be formed on the less steep side, and the sediment will go partly into the sea to be deposited along the shore line, and partly on to the lower lands. This will depress that portion, and it will sink; and the whole tract will be more or less tilted down on that side, and up on the other, because it will turn about an axis through its centre of gravity, which will be situated somewhere beneath the ridge. The ridge also, owing to the great downward protuberance, will stiffen the tract, and give it rigidity to bear this tilt. The tendency will be for fissures to form, chiefly along the shore line on the steeper side of the ridge. The depressions, in which the deep water lies, will also by the same action be moved further away from the ridge on both sides.

Among the causes, which may affect the equilibrium of the crust, overflows of basaltic rocks must be included. But, if we take the substratum to be of the density of basalt, it is easily proved that the level of the tract would be unaltered by the mere transference of a spread of basalt from beneath to the surface. The uplifting of an area, which appears to have frequently accompanied the extensive outpouring of lavas, must therefore be attributed to some kind of energetic upswelling of the molten substratum beneath it.

XIV. We apply to the downward protuberance of the crust

into the substratum, under any elevated tract, the popular expression of "roots of the mountains". The existence of these roots of the mountains are not a mere matter of speculation. They have been felt by aid both of the plumbline and of the pendulum. The great mass of the Himalaya mountains was, during the Indian Trigonometrical Survey, found to attract the plumbline. But upon its being calculated how much attraction ought to be attributed to the mountainous mass, it was found that, though they attracted the plumbline, yet they ought to have attracted it still more than they did. Sir G. B. Airy explained this anomaly by assuming the existence of downward protuberances of a lighter crust into a heavier substratum; which is exactly the same supposition to which our reasoning has just led us. This then is a strong confirmation of our theory of the constitution and arrangement of the crust.

Another point to be observed is, that the floatation of a crust, thus dipping downwards into a heavier liquid at the places where it rises upwards into the air, precludes the transmission of any unequal stresses to the parts below, and renders unnecessary the supposition of extreme rigidity, either in the crust itself or in the subjacent matter, in order to support such stresses.

XV. In like manner as the plumbline shows that there is a deficiency in the horizontal attractive force of the Himalaya mountains, which may be explained by a downward protuberance of the lighter crust into a more dense substratum, so the investigation of the vertical force of gravity in that region has been found to corroborate the same theory. Extensive observations with the pendulum have been carried out to test this question, and the result fully confirms the previous conclusion. At all the elevated stations a deficiency or negative variation of gravity was detected, while at Moré, at the height of 15,408 feet, the pendulum made 21 swings *per diem* fewer than it would have done, had the density beneath the mountains not been in defect.

An important consequence of the existence of these roots to mountainous regions will be, that the elevation of the level of the sea in their neighbourhood by the attraction of mountains

will be quite small, instead of being as formerly supposed sufficient to have considerable geological importance.

XVI. Again, the existence of the roots of the mountains ought to be revealed by phenomena of underground temperature; and we find such to be the case. Whether the crust be thick or thin at any given locality, the temperature of its under surface must be the melting temperature. This temperature may practically be considered the same everywhere. Accordingly any difference in the rates of increment of temperature in descending at two localities ought to depend only upon the thicknesses of the crust, and upon the mean surface temperatures; and the rate ought, as a rule, to be greater in low lying than in elevated regions. This is known to be the case. Careful observations of temperature were made during the construction of the tunnel through Mont S. Gothard, and sufficiently reliable ones also at Mont Cenis. In both these tunnels the rate was found to be about 1° F. for 100 feet, which is only about half the usual rate.

The rate being known, and the height of the mountain, and also the rate at a place elsewhere near the sea level, if we assume the relative densities of the crust and substratum to be those of granite and basalt, we can calculate the thickness of the crust and the melting temperature from these data, without making any assumption about the melting temperature. In this way we have obtained for the thickness of the crust about 25 miles at the sea level; and for the melting temperature about 2500° F. The first of these values agrees with that already estimated in XIII.; and the latter is by no means improbable from what we know about the melting temperature of silicates. The thickness of the crust beneath the mountains comes out about 45 miles.

The two lines of argument in the two preceding and the present chapters, so diverse in their characters and yet pointing in a like direction, are drawn from observations made in regions as far apart as the Himalayas and the Alps; the conclusion from both being the same, namely that there is a protrusion of the lighter material of the crust into the denser subjacent liquid beneath the mountains in both these regions.

XVII. The larger proportion of the rocks which come under our observation having been deposited beneath seas, it might be natural to suppose that the crust beneath the deep oceans is of the same character. This however would be a hasty generalization, especially when we bear in mind that most of the strata we know, thick as they often are, have nevertheless been deposited in comparatively shallow water within the reach of sediment derived from neighbouring lands. It is not improbable therefore that the crust of the earth, once beneath the deep oceans, may never be elevated sufficiently to come under our notice. Indeed many geologists believe that the continents and oceans have never interchanged places, although their boundaries have doubtless oscillated backwards and forwards many times.

The greatest known depth of the ocean is about five miles, but the areas where it is so deep are very limited in extent. The mean depth is about three miles. The prevalence of an envelope of water of less than half the density of the continental crust over about three-fifths of the earth must exercise an appreciable effect upon the aggregate attraction of the entire globe at the sea level. Now it is known from geodetic measurements that, setting aside the ellipticity which is caused by rotation, the earth would otherwise be spherical, and gravity would therefore be constant over the whole.

We have demonstrated in this chapter that an outer shell of uniform depth composed of a crust and a substratum arranged in a certain manner with the addition of the mass of the ocean over the aqueous portions, and constituted as we have already supposed it to be in the continental portions, would produce uniform attraction all over the surface. We suppose also that below this outer shell a spherical nucleus, whether liquid or solid, is eventually reached, consisting of concentric layers each of uniform density throughout. Adding the attraction of this nucleus to that of the shell, our hypothesis provides for the requisite constancy of gravity over the entire surface. The gravitational phenomena of the earth will consequently be satisfied by our hypothesis.

The mathematical results of this hypothesis give us the following information.

(1) The suboceanic crust dips more deeply into the substratum than does the continental crust.

(2) The suboceanic crust is more dense in the lower than in the upper portions.

(3) The lower portion of that crust is more dense than the substratum beneath it.

(4) If the suboceanic crust is of about the same thickness as the continental crust, it must be more dense.

(5) The liquid substratum underneath the oceanic areas is less dense than it is underneath the continents.

Our attempts to determine the actual thickness of the suboceanic crust as compared with the continental have been unsuccessful for reasons duly assigned; but we may reason on other grounds that, if the conductivity of the suboceanic crust is not markedly less than that of the continental, considerations of temperature would lead us to think, that its thickness cannot be much greater than 25 miles, which we have hitherto assumed to be the thickness of the crust at the sea-board. The ice-cold water found at the bottom of the deep oceans, by keeping the surface at the steady temperature of 32° F., would add only about 1400 feet to the thickness of the crust as usually estimated. If it be asked how this estimate can be reconciled with the assertion in (1) that it dips deeper into the substratum than the crust beneath the sea-board, the answer is that the additional dip, or swag, is probably due simply to a bending downward of the bottom of the crust, answering to the *quasi* hollow in the upper surface in which the oceans lie.

The denser layer, referred to in (2), as existing at the bottom of the crust, is probably a thin one, and need not in general render the average density greater than that of the substratum, so that the crust need not sink. But it is possible that, where the ocean attains the exceptional depth of about five miles, this denser layer may have become so thick, that the crust is slowly sinking, and thus the frequent earthquakes may be accounted for, which originate beneath the abysmal depths to the eastward of Japan.

The lesser density, referred to in (5) as distinguishing the substratum under the ocean from that under the continents,

must occasion convection currents between the two, for the substratum cannot be in equilibrium when it is not of equal density at equal depths. Moreover the places where the density is in defect must indicate the site of the rising currents. We learn then that there are currents ascending from the depths below under the oceanic areas. The general result is in strict accordance with the conclusion arrived at in Chapter VI. upon entirely different grounds. We have gained besides the additional information as to the situation of the rising and descending currents, for the latter must necessarily occur beneath the continents, where the substratum is more dense. The immediate cause of such currents prevailing is the heat of the interior, which is everywhere present, and this explains why the area underlaid by the ascending currents is the larger of the two, because the descending currents are merely return currents.

If the ascending currents are to any extent due to vesicularity, the excess of density in the lower part of the suboceanic crust over that of the substratum is what we might expect, because the gas would be extruded upon the gradual solidification of the magma.

Since upward convection ceases at the sea-board, where the currents will become horizontal, there must be some depth of the ocean which corresponds to a maximum play of rising currents. This may probably be indicated by areas that, owing to the great upward pressure, may be slowly rising, so as to form the remarkable plateaux which occupy extensive tracts of the sea bottom. It is on these plateaux that the volcanic islands of mid-ocean are based; and it is obvious that upward currents of the intensely hot magma, pressing against the underside of the crust, is exactly that which would tend to rupture it, and open fissures, and originate volcanic vents.

The above conclusions evidently confirm the theory of the permanence of ocean basins, because it is difficult to conceive how the subjacent crust, once more dense, can have subsequently passed into the less dense condition, which characterises the continental crust.

In the sixteenth chapter we obtained estimates of the thick-

ness of the crust and of the melting temperature, both of which came out smaller than was expected. These estimates were based upon the assumption that mountains are supported in exact hydrostatic equilibrium by their roots projecting downwards into the denser substratum. Convection currents however render the hypothesis of exact hydrostatic equilibrium untenable; for the equilibrium would be slightly affected by the currents. If, as seems now probable, the currents descend beneath mountain ranges, this would have the effect of increasing the values both of the mean thickness of the crust and of the melting temperature, thus affording a presumable corroboration of the results of the present chapter.

XVIII. Having discussed the gravitational relations of the oceanic and continental areas, and seen what conclusions we are led to form as regards the general constitution of the crust beneath the oceans, it seems proper to notice the observations which have been carried out with respect to gravity upon islands. It had long been known that gravity at island stations is greater than on continents in the same latitudes. The true cause of this however does not seem to have been suspected until it was pointed out by Pratt, who noticed that it is owing to the attraction of the submerged rock, which forms the basis of the island. If an island could be removed, and a pendulum could be swung on board ship where the island had stood, gravity would be found to have its normal value for the latitude, because the ship would have nothing but water between it and the earth's crust. But the island has a column of rock of more than twice the density of water between it and the mean surface of the crust. It is this rock which increases the force of gravity upon the island. M. Faye has given a similar explanation of the phenomenon, and recorded a list of observed variations of gravity on seventeen islands, which we have copied from his paper upon this subject. At all but one gravity is in excess. This exception is in the case of the Falkland Islands, which unlike the others consist of disturbed strata of Silurian age. We have explained how this anomaly is fully in accordance with our theory of mountain formation.

XIX. The geological conformation of continents, gathered

as they are about mountain chains, leads to the conclusion that there is an intimate connection between the elevation of continents and compression, and since there is good reason to believe that ocean basins have been permanent, it appears probable that the suboceanic crust has never been subjected to compression. If therefore we desire to estimate the amount of compression of the earth's surface, we feel justified in confining our attention to the continental areas.

Referring to the two modes in which compression may have caused elevation, namely, by either raising the crust into anticlinals into which the subjacent denser fluid would rise, or by producing ridges on the surface which would be accompanied by corresponding depressions of the lighter crust into the liquid beneath, we remark that, if the former were the arrangement, the two phenomena we have lately discussed would be absolutely reversed. For, if the heart of the mountain range consisted of the denser matter of the substratum, its attraction would be greater than if it consisted of matter of the mean density of the crust, whereas the attraction is found to be less. And again, if the molten magma rose up into the base of the range, the increase of underground temperature would be greater in mountainous regions instead of being, as it is, less.

Compression may have arisen from contraction of the interior, or from expansion of the crust, or from these two processes going on together. We have shown in Chapter VIII. that the contraction of a solid earth through cooling cannot explain the existing compression. In an earth so constituted there would always be a certain level, at which compression would cease, and below it give place to extension; and this level of no strain is so near the surface, and the compression above it so inconsiderable in amount, that the resulting corrugations would be extremely small. If on the other hand the interior is liquid, the bottom of the solid crust will be itself a level of no strain, because, when the magma there solidifies on to the lower side of the crust, it will form a layer of the exact size appropriate at that time. It seems probable also that there will be a second level of no strain higher up, due to the mode of cooling of the solidified crust, much in the same way as in a solid earth. On the whole

then it does not appear that any greater amount of compression through contraction from cooling can be obtained on the hypothesis of a liquid substratum than upon that of a solid earth.

Any attempt to estimate the amount of compression, which has affected the crust of the earth, is open to much uncertainty. Even if, as suggested, we suppose compression to be confined to the continental areas, we cannot assert that they have been raised above the sea level solely by compression, because there are considerable areas which appear to have at various epochs undergone vertical uplift. Neither do we know what level of reference is most proper to be taken, above which to measure the elevation of the land. This much however we are able to do. We can estimate what amount of compression would be implied in the supposition that the surface of the land, as we now see it, was once all at the sea level, and that it has been elevated by compression alone to its present height. Allowing for the protrusion of the lighter crust downwards into the denser substratum, by which means it is supported in approximate hydrostatic equilibrium, and taking the densities of the two as being those of granite and basalt respectively, we have found that with a crust 25 miles thick the mean linear compression, which the continents must have undergone to raise their surface to their present mean height of 1000 feet, must have been about 4 per cent.; thus reducing 104 miles run to 100.

The extravasation of water from the interior by means of volcanic eruptions cannot have been an appreciable cause of compression. For even if the whole ocean had been accumulated in that way, and if the water-substance while in solution in the molten magma, had the same volume which it has now, it would have caused an amount of compression amounting to only six per ten thousand. This would be too small to have produced any noticeable elevations.

XX. The stresses which have acted upon the rocks of the earth's crust have occasioned disturbances in them which may be classed under four heads, (1) vertical changes of level, (2) folding of strata on a large scale, (3) shearing of the beds

over one another accompanied by folding on various scales of magnitude graduating into schistosity, (4) faulting.

(1) Of the above, vertical changes of level seem to be the most universal, having affected both continental and oceanic areas, and are known to be in progress at the present time.

(2) In the more simple type of mountain ranges, such as the Jura and Appalachians, lateral pressure appears to have acted in a more direct manner, inducing compression and a consequent thickening, or bulging, of the crust in some such way as indicated in Chapter XIII, greatly reducing the horizontal dimensions of the compressed beds.

(3) The third mode of disturbance consists in a shearing action, and presents some interesting features, which we have attempted to explain, because the distinction between the effects of a direct pressure and of a shearing stress seems only beginning to be appreciated by geologists.

A viscous shear is an action by which the material affected is constrained to move with an increasing velocity the further we recede from some given surface. The movement of water in a deep channel affords a good instance. The water at the bottom of the channel, where it is in contact with the ground, barely moves at all. But the motion becomes more and more rapid at higher levels, and is fastest at the surface. The movement of ice in a glacier is of a similar character. In many instances great rock masses have yielded slowly to stresses, so directed as to have produced in them movements of this nature. The direction of the shear may have been at various inclinations to the horizon according to the direction of the forces acting, but the same directed stresses which could produce a viscous shear, in which the continuity of the material was not broken, would also be competent to produce a fault along a surface of discontinuity. Many instances are met with, where these two kinds of movement are mingled, and in such cases the faults are denominated "thrust planes." Whether such a fault shall be produced, or a viscous shear, will depend upon the ductility of the rock affected and upon the friction; and the friction will depend upon the pressure normal to the shearing planes. If the pressure is sufficiently great, it will increase

the friction, so that it will not allow of faulting. But if the pressure is not too great, a slip may be produced along a surface of separation, and a fault, or thrust plane, will take the place of a viscous shear in satisfying the stress.

The crumpling, or "over folding," of the laminæ, usually attributed to an unexplained action called "earth-pressure," or "crust-creep," is really the effect of viscous shearing in a rock consisting of alternations of harder and softer layers. Following Dr Stappf, we have explained how this crumpling is produced, and have also shown, how the noticeable thickenings and thinnings of the laminæ of the folds in their several parts are caused.

Further, we have explained how a crumpled rock may pass into a schist, and also how the same amount of shear, which at one locality has produced a crumpled rock, may at a locality not far distant produce a schist, without the necessity of an intermediate stage of crumpling.

We have also pointed out what we believe to be the distinction between a schist and a true slate. The cleavage in a schist follows the planes of shear, and is produced by the abrasion of the constituents of an unhomogeneous rock, which are thereby reduced to flattened particles. The cleavage of a slate on the other hand does not follow the planes of shear. It is due to the deformation of the constituents of a more homogeneous and ductile rock, and it follows the flattened faces of the deformed particles. In illustration of this we have given a diagram, which by a simple geometrical construction shows very clearly, how accurately our hypothesis gives the direction of the cleavage in some instances of which Professor Hughes has given sketches.

(4) Faults, of the class where the beds have slidden down an inclined plane of disruption owing to their own weight, are no doubt due to contraction of the rocks in which they occur. The faulting enables a portion of the crust to occupy the same horizontal area after contracting, as it did before, without gaping fissures being formed[1]. But although it is the horizontal

[1] See diagram, p. 89.

contraction which gives occasion to the fault, yet it is the vertical contraction that governs its direction, otherwise faults would not have the rectilineal character which distinguishes them, but would be arranged in polygons.

All readjustments of the crust, which do not arise from horizontal thrust, will in general be effected by faulting. Increase in the weight of an area, decrease in the weight of an area, upward pressure or loss of support from changes in the movements of the substratum—all these would be satisfied by faulting. And, with a crust of no more than 25 miles thick, it is quite possible that some of these causes might originate faults, which would cut right down to the molten substratum, and give rise to volcanic eruptions at numerous points along their course, deluging the surface with successive sheets of igneous rock.

XXI. We have examined two conceivable causes of compression of the earth's surface, originating in contraction of the interior, namely, cooling, and extravasation of water-substance from beneath the crust. Neither of these appears to be adequate to produce the amount of compression which exists. No other cause of contraction of the globe suggests itself.

We next look about us for a possible cause of extension of the surface, which, it is obvious, would equally with contraction of the interior, produce compression. The numerous more or less vertical dykes of igneous rock lead us to inquire, whether the rending of the fissures which they occupy may not be indicative of the extension of which we are in search. The pressure of the crust upon the liquid substratum is about 10,000 tons upon the square foot. If the water-substance from the magma were to escape into a crack in the under side of the crust, it would exert this pressure towards rending it wider; and so long as the vapour or gas was supplied with sufficient rapidity, it would prevent the magma from rising into the chasm so formed. When the rent reached the surface, the vapour would rush forth and be followed by the magma itself, now appearing as lava; and thus a volcano would be established. But this would be an exceptional occurrence. It would be only here and there that the vapour would escape at the surface, because its doing so

at one point would relieve the internal pressure for a long distance.

The above described process appears to be sufficient to establish a volcanic vent, and agrees well with the series of earthquakes which usually for a long while precede an eruption, and very often occur without any eruption at all. Nevertheless permanent elevation of the tract is said to be a common result of earthquakes.

We have illustrated the actual origination of a volcano out of a dyke by a diagram copied from Dutton's history of the Cañon district.

As regards the compressive effects of the injection, consider the case in which the magma itself in a molten state is pressed up into the chasm by the weight of the crust resting upon the surface of the substratum. If it was an inert liquid of about the density of the crust, it would have no power to rend the chasm wider; but since it carries a compressed gas at a high temperature along with it, this will enable it to exert a strong pressure on the cavity in which it is contained. The amount of this pressure will depend upon the solubility of the gas. We have calculated the pressure that the lava would exert at any given height in the column: and on the supposition that the gas is about as soluble in molten rock as carbonic acid is in water, or even half of that, we find that, if a column of lava is working its way upwards through the crust, it would press upon the rock above it with a powerful thrust. Even at the surface of the ground this thrust would be very great, and would account for such an event as occurred at Santorin in 1866, when the sea bottom was lifted up into a dome, from which blocks of rock fell away as it rose, until red hot masses were protruded, and at last the lava itself appeared. A like explanation may be given of the formation of horizontal masses of intrusive rock, which have lifted the superincumbent beds as in the case of the Whin Sill and of laccolites.

The mean horizontal pressure upon the entire side of a chasm reaching from the substratum to near the surface, and tending to rend it open, would, upon the same suppositions as to the solubility of the gas, be comparable to the weight of a

column of rock of from two and a half to one and a half miles high. This would clearly exert an appreciable compressive force upon the neighbouring crust, and be competent to deform the rocks. As far as their weight only is concerned, it would be capable of raising an elevated tract of the height of from five to three miles above the mean level of the crust, but not higher. Now the mean level of the crust lies beneath the sea level, and many mountains are higher than indicated, even above the sea level; so that we cannot credit this cause of compression with all the work done, especially when we take into account the work required also to deform the material. Still it is a matter of observation, that much fissuring of the crust, accompanied by volcanic activity, has been contemporary with the throes of elevation of great mountain chains.

Chasms opening from below, and admitting the expansive magma, might be expected to be formed, where thick deposits of sediment were depressing the crust into the substratum. A certain amount of compression would arise from its expansion. But that cause of compression, as just stated, does not appear to be sufficient to account for the entire result, which seems to require a more extensive and more energetic agency.

XXII. It used to be thought, as was natural, that the seat of volcanic energy resides in a substratum of molten rock underlying the cooled crust of the earth. But when Hopkins published what for a long time was thought to be a proof of the very great thickness of the crust, he suggested that volcanic vents were connected with separate subterranean lakes of lava. This opinion was endorsed by those physicists, who corroborated Hopkins' theory of solidity. But the idea of separate lava lakes did not appear satisfactory to geologists, and at this juncture Mallet published his ingenious theory of volcanic energy. He asserted that vulcanism is the result of heat developed during the crushing of the superficial strata by means of the compression which had been attributed to the contraction of the earth through secular cooling. We have however proved the insignificant amount of this contraction, which could not possibly produce any appreciable movements of the crust during such

times as history embraces. Yet volcanic phenomena are of daily occurrence.

Mallet based his theory upon the results of experiments in crushing cubes of rock, from which he *calculated* the amount of heat obtainable in that way, and concluded that the whole of the heat, that would arise from crushing one cubic foot of rock, would fuse about one-tenth of a cubic foot of the same. Supposing the heat so obtained could be localized, he considered that the crushing of rock within the crust of the earth would originate volcanic action. He assumes with Hopkins, that the crust is thick, and postulates 400 miles for the thickness. We however show that the heat obtained could not be localized, but must be distributed through the crushed portion, appearing only where crushing takes place, and appearing everywhere in proportion to the work done there; so that if, say, ten miles of the crust was crushed, it could not fuse one mile selected out of those ten.

Mallet's experiments however could not have represented what really occurs deep in the earth's crust; and even if they had done so, the heat obtainable under the hypothesis would have been quite inadequate to produce the phenomena which occur.

The theory we have been describing was an attempt to obtain by mechanical means what geologists have seen to be necessary, and have endeavoured to account for in several ways, namely a source of intermittent increments of heat beneath volcanic areas. This however appears to be furnished by the circumstance that we have discovered, namely the existence of convection currents in the molten substratum, which underlies the crust of the earth. We have proved that, if the crust is as thin as geological phenomena point to its being, these currents must bring up liquid, so far as temperature goes, sufficiently hot to fuse the solidified crust against which they impinge. The reason why the crust on the whole grows thicker is, because the heat so imparted is conducted through it, and radiated away into space more rapidly than it is supplied. But it is in the nature of convection currents to move with what seems like irregularity, although really governed by definite laws, if

we only could trace out their action. Should the result be that the currents become periodically more energetic beneath certain regions, these will be rendered volcanic, because they will be subject to occasional remelting and thinning of the crust.

Still further, the constitution of the magma, holding water-gas and other gases in solution in accordance with Henry's law, offers an explanation of the phenomena more immediately attending a volcanic eruption. When the lava is rising in the chimney of a volcano, the vesicularity produced by the liberation of these gases under reduced pressure lessens the mean density, and enables it to reach the surface, where ebullition and explosion testify to the mode of action, and much of the lava is eventually blown into dust.

The fact that the gases are extruded upon solidification, leads us to believe that, as the crust thickens, the equivalent gases which were previously dissolved must be set free, and must in some way or another find a mode of escape. This may ordinarily be by an unnoticed absorption among the rocks, assisting the elevation of temperature which is indicated by the gradient. In volcanic areas however, where the action is more energetic, solfataric action may be the indication of the escaping gases. It is even conceivable that these gases may occasionally accumulate in hollows in the underside of the crust, where it is thin, and give rise to those explosions, which are known sometimes to occur.

We have attempted to explain the usual sequence of events attending a volcanic eruption in accordance with our hypothesis: but for the details of this part of the subject the reader is referred to the body of the book, because they are not of a technical character, and cannot be easily epitomized.

XXIII. We have described the scale of magnitude of the disturbances which have affected the strata of continental areas, and have shown that compression is probably confined to those regions, and to islands which have been once connected with them. Hitherto however our endeavours to discover the cause of the greater disturbances have yielded results mostly negative. It is clearly impossible that the weight of sediment by depressing the rocks at one locality could raise them at another

to a higher level than its own, and this could not be higher than the sea level where the deposition was going on. Hence the elevation of contorted strata above the sea level cannot be explained in that manner. We have seen that the contraction of the earth as a whole cannot account for the compression either in amount or in distribution, and that volcanic action, though it may have contributed something to the grand result, can only have played a subordinate part. But the discovery of the fact, that a thin crust implies convection currents in a truly liquid substratum, affords a new and perhaps sufficient explanation of the phenomena. Now the calculations in the seventeenth chapter lead to the conclusion, that the ascending currents are situated beneath the oceanic areas and the descending currents beneath the land. Consequently the liquid magma must flow from the oceanic towards the continental areas, and must acquire a more or less horizontal motion as it approaches the main coast lines. It is clear that this will tend to press the supernatant crust from the oceanic towards the continental areas, and to produce compression along their common boundary. As soon as compression begins to take effect, roots to the elevated portions will be produced, which dipping into the substratum will offer an increasing obstacle to the flow of the currents, and intensify their operation. Thick deposits also, by depressing the crust into the liquid would have a similar result, and thus encourage local compression.

Although the movement of the currents is no doubt slow, yet it must be remembered that the density of the material is greater than that of the crust on which it operates. The action would resemble that which we may notice when cakes of ice float down a river, and impinge upon an icy barrier, where, their edges tipping down into the influence of the stream, the blocks get pushed over one another, and piled together, much like strata dislocated by thrust planes.

Where the currents ascend beneath the ocean they would give rise to a tensile stress, the correlative of the compression of the land. Fissures would thus be produced, which would open volcanic vents, and, when filled with solidified lava, become dykes of igneous rock in the suboceanic crust. Such fissures

however, owing to the mode of their formation, would not be accompanied by compression. At the same time the varying intensity of the rising currents would cause an instability of the ocean bottom, such as is evidenced by marine deposits being found at various altitudes upon the volcanic islands of mid-ocean.

Although the presence of convection currents renders the hypothesis of hydrostatic equilibrium no longer exact except at the places where they move horizontally, it is not at all probable that they are sufficiently rapid to interfere materially with the consequences of that hypothesis.

Now the peculiar arrangement which is requisite for the equilibrium of a disturbed crust resting upon a heavier liquid substratum is, that, for every subaerial elevation above the mean surface, there must be a corresponding protuberance, dipping downwards into the liquid below; and according to the relative densities which we have assumed, the depth of these protuberances must be about ten times the height of the elevations. We have already seen that this circumstance explains certain remarkable phenomena, connected respectively with mountain attraction and with underground temperature. We now proceed to show that it also agrees extremely well with many of the well-known geological movements of the surface. As the surface is denuded down, it will be concurrently raised up from below by floatation, and the immense quantity of material denuded off a tract will not require the tract at any one period to have had the altitude, which the amount of denudation at first sight appears to necessitate: nor yet will any additional exciting cause of elevation be required, beyond the mere fact of the denudation. The course of drainage across the dip is also explicable on this hypothesis.

This explains how it happens that, when the original contours of the strata are restored, the amount of denudation, which they can be proved to have undergone, can have been so enormous, without its being necessary to believe that the surface ever stood at the height above the sea, which those contours seem to indicate: for it is much less difficult to conceive them to have been gradually lifted up concurrently with, and as a consequence of, their denudation, than to suppose

that they were once thousands of feet higher than they are at present. The immense amount of sediment which has been derived from regions, which remain still very lofty, is also explained by the same process. Changes of level in high latitudes may be accounted for on the same principle, as arising from the secular increase or diminution of the extensive snow fields which cover the surface in those regions.

XXIV. The geographical distribution of volcanos presents fewer difficulties upon the supposition of a thin crust and liquid substratum, than upon any other. The linear arrangement of many of them points to their being situated along great systems of fissures; and such systems of fissures are indicative of a thin crust. Fissures, which run for long distances in nearly straight courses, point, as already mentioned when discussing faults, to a movement perpendicular to the fissured surface; or else they point to a rending pressure within the fissure itself; while on the other hand fissures, which are caused by contraction in a direction parallel to the surface, would divide up an area into polygonal figures. The former arrangement of the fissures accords best with the distribution of volcanic ranges, and suggests a thin crust.

We recognise two principal types of volcanic regions, coast-line and oceanic. We believe the former to be connected with the agencies which have raised the continents which they skirt. Trains of such vents are attached laterally to the great compressed and elevated coast ranges, and usually stand near the edge of a steep shore. The oceanic volcanos on the other hand appear unconnected with compressive action, for the oceanic islands consist almost all of them of volcanic rocks: whereas, if they were connected with mountain ranges, the peaks of schistose or other hard inclined strata could not well be absent: moreover fragments of stratified rocks have not been found among the ejecta of these volcanos.

They occupy a medial position with respect to the coast lines, being in the Atlantic rudely parallel to the opposite shores, and in the Pacific always active in the central patch of the Hawaiian Islands.

Volcanic cones, having no roots projecting downwards into

the substratum to support them, would sink, and, rupturing the crust around them, tend to perpetuate volcanic activity in the same region. The fissures would be formed around the cones, and thus a train of volcanos, originally linear, would give occasion to an elliptical ring of vents to which the peculiar shape of some of the coral archipelagoes may possibly be due.

The existence of some kind of connection, between volcanic action and the elevation of continents, is indicated by the remarkable course of the great volcanic band which borders the Pacific ocean: for it approximately divides the world into two halves, one of which contains nearly all the land and the other, except Australasia, is covered with water. But the course of this great band is turned aside by its union with another, which comes from the North-West through Sumatra and Java; and the exceptionally situated insular continent of Australia stands in relation to the deflected continuation of the same train of volcanos, much as Asia does to the previous portion of it. No other cause less than one of a very general character, possibly cosmical, can have produced this unique separation of the earth's surface into land and water, accompanied by a continuous band of fissured crust round their junction.

Our theory of volcanic action needs only the opening of fissures which shall reach the fluid substratum, without reference to how those fissures may be caused. We think therefore that the present localization of volcanic activity, at interrupted regions along the flank of an elevated range, may be traced to the deposition of sediment, even at a considerable distance, upon the side of the range opposite to that on which the volcanos lie. An inspection of our diagram on page 184 will explain how this would be: and we have offered some reasons for supposing that this suggestion is supported by geographical facts.

XXV. The occupation of an entire hemisphere by one great ocean is a remarkable circumstance, and we have seen good reasons for believing that this is a very ancient division of the surface, and that it is probably a mistake to suppose, that every part of it has been sometimes raised above the sea,

and sometimes depressed beneath it. The truth seems to be, that the region subject to these alternations of conditions does not extend very far away from the present coast lines. Our calculations based upon attraction have led to the conclusion, that the crust beneath the oceans is more dense than it is beneath the land, but on the other hand the liquid magma again beneath that is not so dense as beneath the land. This must lead to there being ascending currents beneath the oceanic areas.

We have also seen reason to infer that the crust beneath the oceans has not been compressed into elevated ranges, as it has been in many parts of the land. Still further, we have seen that the oceans lie in veritable depressions or basins, below the spheroidal surface of earth, as if they have been hollowed out.

If the ocean basins have been hollowed out, how was the material removed, and whither has it gone? Professor Darwin's theory of the genesis of the Moon suggests an answer to this question. If, as his investigation renders probable, the moon broke away from the earth fifty million or more years ago, it is worth while to inquire whether the formation of the ocean basins may not have been the consequence of that catastrophe.

The mass of the moon is such that a cavity about forty miles deep, extending over the oceanic areas, would have supplied the requisite material. The density, singularly enough, would have been just right. The matter removed would no doubt in time conglomerate into the lunar sphere, and so we may bid adieu to our satellite. Returning to the earth, we inquire what would happen here. If that was still wholly liquid, or even plastic, the cavity would no doubt soon be obliterated. But if there was already a crust formed, thinner of course than it is now, but yet thick enough to hold together, the part of this crust which covered the place of the cavity would probably go away with the rest of the matter removed. The hole would be filled up by the influx of the molten substratum from beneath and around. The remaining crust would separate into larger and smaller fragments, and partly float towards the cavity. Thus when the newly exposed surface of the molten substratum again solidified,

a fresh crust, of greater density than before, would be formed out of the heavy substratum over the middle of the area, where the hollow had been made, and also in the channels between the fragments which had floated towards it; the Atlantic being the chief of these channels.

Thus would have been formed at an early period of the earth's history a separation into oceanic and continental areas. The rise of the substratum into the cavity from which the moon's substance was detached, would determine the upward convection currents to those areas, and by the action of these, combined with the other agencies of which geology takes cognisance, the present state of things on the earth's surface would have been gradually evolved.

It is no valid objection to this speculation, but rather the reverse, that the centre of the oceanic hemisphere does not now lie on the equator, as it must have done at the moment of rupture; for the sudden removal of so considerable a fragment from the rotating spheroid would alter its mass, form, and moment of momentum; and the axis of rotation would assume a new position within the mass.

INDEX.

[The numbers printed in brackets refer to the Summary.]

Cambridge:
PRINTED BY C. J. CLAY, M.A. AND SONS,
AT THE UNIVERSITY PRESS.

APPENDIX

TO THE SECOND EDITION OF

PHYSICS OF THE EARTH'S CRUST.

BY

THE REV. OSMOND FISHER, M.A., F.G.S.,

RECTOR OF HARLTON, HON. FELLOW OF KING'S COLLEGE, LONDON, AND
LATE FELLOW AND TUTOR OF JESUS COLLEGE, CAMBRIDGE.

London:
MACMILLAN AND CO.
AND NEW YORK.
1891

Cambridge:
PRINTED BY C. J. CLAY, M.A. AND SONS,
AT THE UNIVERSITY PRESS.

PREFACE TO THE APPENDIX.

THE three appended chapters have been written for reasons given in each of them. The first and last of them were called for by criticisms which appeared when the second edition of the book was published. The XXVIIth embodies the results of experiments undertaken at my solicitation. I believe that the entire subject has been made more complete by what is now added. My readers may nevertheless need to be reminded, that the results of all mathematical investigations, although presenting the legitimate outcome of the hypotheses from which they start, possess no greater authority than the original hypotheses, and of the validity of these they must judge for themselves. At the same time it seems that the whole of my theory hangs so well together, and so fairly accounts for many geological phenomena previously imperfectly understood, that the underlying assumptions cannot be very far from the truth.

Mr Brill has kindly continued his assistance throughout.

OSMOND FISHER.

HARLTON RECTORY,
September, 1891.

ADDITIONAL ERRATA IN THE SECOND EDITION.

Page 75, line 1, *for* $\mu = \sqrt{4\kappa t}$ *read* $k = \mu\sqrt{4\kappa t}$.

Page 95, line 19, *dele* dx.

Page 96, lines 2, 8, *for* $\substack{> \\ = \\ <}$ *read* $\substack{< \\ = \\ >}$.

Page 238, lines 6, 7, *for* $-(\sigma - \rho')$ *read* $+(\sigma - \rho')$.

Page 308, line 3, *for* or *read* of.

CHAPTER XXVI.

"THE SUBOCEANIC CRUST." FURTHER CONSIDERATIONS.

Mr Jukes-Browne's criticism on Chapter XVII.—The Argument recapitulated—Four conditions of the crust examined—A fuller investigation of the accepted condition—A correction found necessary—Geological explanation of the new result—Change of conditions near the shore line—Mr Jukes-Browne's hypothesis stated—Examination of the same—It can only be received with a modification which reduces it to that already accepted—Geological consequences of the general result—Dana's views on oceanic topography—These are in accordance with the results now obtained.

SHORTLY after the second edition of "The Physics of the Earth's Crust" was published, a long and highly interesting letter from Mr Jukes-Browne appeared in "Nature[1]," which was in effect a criticism upon Chapter XVII. wherein the constitution of the Earth's crust beneath oceans is discussed. The appearance of that letter has rendered it desirable to reconsider this part of the subject.

The especial points to which Mr Jukes-Browne has invited attention will also be noticed.

To recapitulate the line of argument followed in the XVIIth chapter, we observe that, if it was known that at various marine localities the variation of gravity from a constant value was simply that arising from the ellipticity and the rotation, it would be a proof that, setting aside these, the surface of the ocean must be truly spherical, and not affected by local irregu-

[1] "Nature," Nov. 21, 1889, vol. XLI. p. 54.

larities. This would at once follow from Sir G. G. Stokes' discussion of Clairaut's law. Now Pratt, and more recently M. Faye, have explained that the observed excess of gravity upon islands is sufficiently accounted for by the density of the rock, on which an island stands, being greater than that of the water which it displaces; and M. Faye's calculation shows that, when this is allowed for, it brings the observed daily number of vibrations of the second's pendulum, at most island stations, to within one or two of the number proper to the latitude in accordance with Clairaut's law[1]. But the depression or elevation of the surface of the ocean below or above the mean sphere, which would correspond to one vibration in excess or defect *per diem*, would be 242 feet; and this is quite inconsiderable compared with the variations in depth of the ocean. We are therefore justified in regarding the sea level, setting aside the rotational effects, as being sensibly spherical. Now seeing that the density of sea water is less than half that of rock, this fact indicates that the arrangement of densities beneath the oceans must differ materially from that beneath the surface of flat land at the sea level. It may be remembered that the parts of continents, which are as much elevated above the sea level as the mean sea bottom is depressed beneath it, are of very limited extent.

We have found certain mathematical relations which must subsist among the densities and thicknesses of the outer layers of a sphere, whose deeper portions consist of spherical concentric layers each of uniform density throughout[2]. If we assume such to be the state of the deeper parts of the earth, we may, by means of the equations expressing these conditions, compare the densities and thicknesses of the ocean, and of the layers of the crust beneath it, with the densities and thicknesses of the layers of the crust at the seaboard, and note the consequences which result from any hypothesis that seems reasonable that we may choose to make respecting them. We have done this already under four conditions.

[1] "Comptes Rendus," 22 Mar. 1886. See Chap. XVIII.

[2] p. 235, *et seq.*

First. We tried the hypothesis that the crust beneath the land consists of a single layer, and that beneath the ocean also of a single layer, its thickness and density varying with the depth of the ocean; and that under both regions the substratum maintains the same density.

This assumption was found to fail.

Secondly. We assumed as before with this difference, namely, that the density of the substratum is not the same beneath the two regions.

This assumption also was found to be untenable.

Thirdly. We then examined the case in which the continental crust is supposed to be made up of two layers differing in density, while the suboceanic crust consists of one layer only, the density of the substratum differing under different areas. This hypothesis also failed.

Fourthly. We supposed the continental crust to consist of a single layer, and the suboceanic crust of two layers differing from one another in density, while the substratum was of different densities under different areas. There was not found any reason why this assumption might not be near the truth, and it was therefore accepted.

It appeared that, when the equations were adapted to this fourth hypothesis, they led to the conclusion that, if we are to have the density of the lower layer of the suboceanic crust greater than that of its upper layer[1], the total thickness of the crust beneath the oceans will be greater than beneath the land at the seaboard, and will therefore dip more deeply into the substratum. But, unfortunately, this proviso was not reintroduced in the "summary;" and the greater thickness and the deeper sagging were taken to be ascertained results[2].

We now however proceed to show that a fuller investigation of the hypothesis proves that the arrangement of densities beneath the ocean are in fact the reverse of this, for the density of the upper layer must be greater than that of the lower; and

[1] "If we are to have ρ_2 greater than ρ_1." p. 245.

[2] p. 364.

the result follows, that the suboceanic crust is the thinner of the two, and also dips less deeply into the substratum.

μ, δ	
ρ_1, k_1	ρ, c
ρ_2, k_2	
σ', x	σ, $\delta + k_1 + k_2 + x - c$

We have on the side of the ocean sea water of the depth δ and of density μ, a crust consisting of two layers of densities and thicknesses respectively ρ_1, k_1, and ρ_2, k_2, and a substratum of density σ' and unknown depth x. On the side of the land we have a crust consisting of a single layer of density ρ and thickness c, and a substratum of density σ and of depth

$$\delta + k_1 + k_2 + x - c.$$

Referring to p. 244 we see that the relations between the thicknesses and the density differences are then given by the four equations

$$(\rho_1 - \mu)\,\delta k_1\,(k_1 + k_2)\,(k_1 + k_2 + x)\,(c - \delta) \quad \text{......(1)},$$

$$= (\rho_2 - \rho_1)\,k_1 k_2\,(\delta + k_1)\,(k_2 + x)\,(\delta + k_1 - c) \quad \text{......(2)},$$

$$= (\sigma' - \rho_2)\,k_2 x\,(k_1 + k_2)\,(\delta + k_1 + k_2)\,(c - \delta - k_1 - k_2) \quad \text{......(3)},$$

$$= (\sigma - \sigma')x(k_2 + x)(k_1 + k_2 + x)(\delta + k_1 + k_2 + x)(\delta + k_1 + k_2 + x - c) \text{...(4)},$$

$$= (\sigma - \rho)c(c - \delta)(\delta + k_1 - c)(\delta + k_1 + k_2 - c)\,(\delta + k_1 + k_2 + x - c) \text{...(5)}.$$

From (1) and (5) we have

$$\frac{\rho_1 - \mu}{\sigma - \rho} = \frac{c\,(\delta + k_1 - c)(\delta + k_1 + k_2 - c)(\delta + k_1 + k_2 + x - c)}{\delta k_1\,(k_1 + k_2)\,(k_1 + k_2 + x)} = s, \text{ suppose.}$$

For $k_1 + k_2$ write $c + \alpha$.

Then $$s = \frac{c\,(\delta + k_1 - c)\,(\delta + \alpha)\,(\delta + \alpha + x)}{\delta k_1\,(c + \alpha)\,(c + \alpha + x)}.$$

This may be written

$$s = \frac{m(\delta - c) + mk_1}{nk_1},$$

and therefore $$k_1 = \frac{m(\delta - c)}{sn - m},$$

where $m = c(\delta + \alpha)(\delta + \alpha + x)$,

and $n = \delta(c + \alpha)(c + \alpha + x)$,

$$\therefore\ k_1 = \frac{c(\delta + \alpha)(\delta + \alpha + x)(\delta - c)}{s\delta(c + \alpha)(c + \alpha + x) - c(\delta + \alpha)(\delta + \alpha + x)} \dots \text{(A)}.$$

Now from the nature of the case k_1 must be positive, and $\delta - c$ is clearly negative. Hence if the denominator is positive, $\delta + \alpha$ must be negative, or α must be a negative quantity numerically greater than δ.

Since μ, σ, and ρ are supposed known, therefore $(\rho_1 - \mu)/(\sigma - \rho)$, or s, can only be increased by increasing ρ_1. If we put $s = 5$ this will make $\rho_1 = 2{\cdot}40$; and since ρ_1 is the density of some kind of rock, probably a dense one, this is as small a value as ρ_1 can have. Therefore s cannot be less than 5. This is therefore the least value we can give it. The question therefore is whether

$$5\delta(c + \alpha)(c + \alpha + x) - c(\delta + \alpha)(\delta + \alpha + x)$$

is positive or negative.

If $\delta = 5$, that is where the ocean is deepest, this is obviously positive; and therefore α is negative and numerically greater than 5. Hence, where the ocean is deepest, the crust must be less than $c - 5$ miles thick.

Seeing then that α is negative where the ocean is deepest, it must be always negative, because if it becomes positive it must pass through zero. But if we put $\alpha = 0$ the value of k_1 would become

$$\frac{c\delta(\delta + x)(\delta - c)}{s\delta c(c + x) - c\delta(\delta + x)} = \frac{(\delta + x)(\delta - c)}{s(c + x) - (\delta + x)}.$$

This would be negative, which k_1 cannot be. Hence α being once negative must always be so. It appears therefore that, if the crust beneath the ocean consists of two layers, the depth of the ocean and of the crust beneath it taken together cannot be

so great as the thickness of the continental crust at the sea level.

Is this physically probable?

On the hypothesis of a cooled crust resting on a substratum in a state of fusion, if the conductivity of the crust beneath the ocean is about the same as beneath the land, a greater thinness would imply that the flow of heat through it is more rapid. This would be the case if the supply of heat beneath it was more rapid, and still more so, if the removal of heat from the upper surface was also more rapid[1]. There seems reason to suppose that both these conditions may be present. We shall see presently that there must be upward currents in the substratum beneath the ocean. This will cause the under side of the crust to thicken more slowly, and will supply heat more rapidly than beneath the land. At the same time heat is probably abstracted more rapidly from the upper surface of the suboceanic crust; because the melting of the polar ice absorbs a large quantity of latent heat, and much of this is doubtless originally contributed from below by conduction through the suboceanic crust, and conveyed to the melting ice by circulation through the main body of the deep oceans, which consists of water at a nearly uniform temperature of about 36° Fahr. This mass of water receives but little heat from solar influence, for the layer so heated floats near the surface, and is carried away by surface currents, the downward conduction of heat through the water being very small. Hence we see that the abstraction of heat from the upper surface of the suboceanic crust is great, and probably in excess of that from the continental crust. Thus both of the conditions for a higher temperature gradient, and consequently for a thinner crust, will be present.

Having now found that $\delta + k_1 + k_2$ is less than c, we perceive that every factor involving thicknesses in our four equations is positive, except the last factor of (2) and the last but one and

[1] The case is somewhat similar to what we see at a smith's forge. The smith brings one end of a bar of iron to a welding temperature of incipient fusion, and meanwhile, by dropping water on the other end, keeps it sufficiently cool to be handled. The two extreme temperatures are thus brought within a shorter distance, and the temperature gradient in the bar is accordingly increased.

the last but two of (5). But since $(\rho_1 - \mu)$ is positive, every one of the products must be positive. Hence we learn that the relative magnitudes of the densities are,

$$\begin{aligned} \rho_1 &> \mu, \\ \rho_1 &> \rho_2, \\ \sigma' &> \rho_2, \\ \sigma &> \sigma', \\ \sigma &> \rho. \end{aligned}$$

It appears therefore that ρ_1 and σ' are the greater densities on the side of the ocean, a layer of less density being included between them; while we have from the beginning assumed σ to be greater than ρ on the side of the land. Moreover we perceive that σ is greater than σ'. Hence, if the substratum is liquid, upward convection currents must exist beneath the ocean.

It follows from the above relations that, unless ρ_1 is considerably greater than ρ, a frustrum on the side of the ocean cannot be of equal mass with a frustrum of the same volume on the land side, seeing that so much of the former consists of water. But this equality of mass is a fundamental necessity (p. 236). Hence the upper layer of the suboceanic crust must have a high density.

Having four equations and six unknown quantities, viz. ρ_1, k_1, ρ_2, k_2, σ', and x, we are at liberty to make two assumptions provided they fulfil the conditions of the problem. Let then one of our assumptions be that

$$\frac{\rho_1 - \mu}{\sigma - \rho}, \quad \text{or} \quad s, = 7.$$

This with the assumed values $\sigma = 2{\cdot}96$ and $\rho = 2{\cdot}68$, μ being the density of water, will make ρ_1 near about 3, a suitable value, because, as we have just seen, we require for it as high a value as we can fairly assign to rock. For our second assumption, since we have now found, contrary to our original idea, that the suboceanic crust does not dip so deeply into the substratum as does the continental crust, if we assume as formerly 25 miles for the thickness of the continental crust we may perhaps fairly put $\delta + k_1 + k_2 = 23$ miles, or $k_1 + k_2 = 18$ miles, where the ocean is deepest, which makes the dip less by two miles than the thickness of the continental crust.

If we were now to introduce these values into our four equations, we should have only four unknown quantities remaining, viz., x, ρ_2, σ', and either k_1 or k_2, and, if the equations were less complicated, these might be determined for those regions.

Nevertheless we can find limits within which k_1 must lie, and these will fix limits for k_2. For if we assume $k_1 + k_2 = 18$, as above suggested, and therefore $\alpha = -7$, we have from (A)

$$k_1 = \frac{100x - 200}{1124 + 68x}.$$

If $x = 2$, $k_1 = 0$; k_1 then increases and attains the limiting value of 100/68 or 1·47 miles when x becomes very large. This shows that the upper layer of the suboceanic crust must in any case be a shallow one.

Such an arrangement of densities and thicknesses certainly needs explanation. The thinness of the whole crust and the upward convection currents point to an exceptionally energetic state of the substratum beneath the deep oceans; and this would be accompanied by frequent volcanic outbursts. May not therefore the shallow and dense layer, which appears to form the upper part of the suboceanic crust, consist of a succession of flows of basic lavas; because the deposit of red clay, which covers the sea bottom in those regions, would not possess sufficient density to satisfy our hypothesis? If this is the case we might expect the density of the upper layer to be nearly the same wherever it occurs, however much its thickness may vary.

Under these circumstances we shall have in any particular region μ, δ, and ρ_1 given, and x also determinate in value because $\delta + k_1 + k_2 + x$ must be an invariable sum. There will remain then only k_1, ρ_2, k_2, and σ' to be determined. And there are four equations. It seems then that these four quantities are determinate; but since we cannot solve the equations we must be content to reason about them as best we may.

From considerations arising out of the necessary equality of the masses of equal frustra beneath different areas we may conclude that, where the ocean is less deep, *i.e.* δ smaller, the mass of k_1 is probably smaller, *i.e.* the upper layer becomes still thinner,

because we suppose its density unaltered. The masses of k_2 and of the substratum would then be greater. The increase in mass of k_2 would probably be due to its greater thickness rather than to greater density. A thinning of k_1 need not diminish its attractive force at the surface of the sea, because where the sea is shallower the mass of k_1 would be nearer, though smaller.

In our expression for k_1 we see that, if α is negative and nearly equal numerically to δ, k_1 becomes small, but if δ is small, that is where the ocean is not deep, for instance not more than a mile, then α being near a mile would make $k_1 + k_2$ a little over 23 miles, which would make the crust thicker where the ocean is less deep. If the equations could be solved we should have certain information whether this is the case, but as they cannot, we must be content with the probability that it is so.

These suppositions would agree with the probable circumstance that the upward currents are less energetic where the ocean is less deep, for that would allow the crust to have become thicker and would also show that the substratum is more dense. If there be truth in the doctrine of the permanency of the great ocean basins, it is probable that as we approach the shore line the geological constitution of the crust undergoes a change, and that there is an intermediate belt of which we cannot assert that it belongs exclusively either to the oceanic or to the continental type. To such regions the present considerations would not apply. In mathematical language we cannot pass from the one type of crust to the other by putting $\delta = 0$.

In order to complete the consideration of the several cases in which we may proceed to the degree of approximation indicated by two layers of rock, we ought to examine the case of two such layers under both areas. And this is the hypothesis proposed by Mr Jukes-Browne in his letter to "Nature" already referred to, which contained the passage—"It is conceivable that the lower part of the crust is *everywhere* denser than the upper part and consequently that two layers of the continental crust should be introduced into the problem: whether this hypothesis would likewise fulfil the conditions, and whether it

would lead to the same results as that which Mr Fisher adopts could only be ascertained by trial."

We have just found that it is necessary to reverse the order of densities of the two layers beneath the ocean, when assuming a single layer beneath the land. Let us now however examine the case of two layers beneath each area as Mr Jukes-Browne suggests.

μ, δ	ϕ_1, c_1	
ρ_1, k_1	ϕ_2, c_2	d
ρ_2, k_2	σ, $\delta + k_1 + k_2 + x - c_1 - c_2$	
σ', x		

Calling c_1, c_2, the thicknesses of the two layers of the crust on the land side, and ϕ_1, ϕ_2, their densities, σ being the density of the substratum on that side, by substituting the proper values for τ, t, &c.[1] we have for our first equation

$$(\mu - \rho_1)\,\delta + (\rho_1 - \rho_2)\,(\delta + k_1) + (\rho_2 - \sigma')\,(\delta + k_1 + k_2) + \sigma' d$$
$$= (\phi_1 - \phi_2)\,c_1 + (\phi_2 - \sigma)\,(c_1 + c_2) + \sigma d\,;$$

or transposing,

$$(\mu - \rho_1)\,\delta + (\rho_1 - \rho_2)\,(\delta + k_1) + (\rho_2 - \sigma')\,(\delta + k_1 + k_2) + (\sigma' - \sigma)\,d$$
$$+ (\phi_2 - \phi_1)\,c_1 + (\sigma - \phi_2)\,(c_1 + c_2) = 0.$$

This consists of six terms, and there are five unknowns, so that the method of resolution into factors is applicable; and, since it contemplates two layers in the continental crust, the hypothesis meets the wish of Mr Jukes-Browne.

The equations are now to be carried to the 5th power of the thicknesses, and thus terms of the high order $(t/a)^4$ are retained, and the approximation will be very close.

Our five equations may now be put into the form

$$(\mu - \rho_1)\,\delta\,(-k_1)\,(-k_1 - k_2)\,(\delta - d)\,(\delta - c_1)\,(\delta - c_1 - c_2) \quad \text{.........(1)},$$
$$=(\rho_1 - \rho_2)(\delta + k_1)k_1(-k_2)(\delta + k_1 - d)(\delta + k_1 - c_1)(\delta + k_1 - c_1 - c_2) \text{...(2)},$$
$$= (\rho_2 - \sigma')\,(\delta + k_1 + k_2)\,(k_1 + k_2)\,k_2\,(\delta + k_1 + k_2 - d)$$
$$(\delta + k_1 + k_2 - c_1)\,(\delta + k_1 + k_2 - c_1 - c_2) \quad \text{...........(3)},$$

[1] p. 235 *et seq.* Ch. XVII.

$$=(\sigma'-\sigma)d(d-\delta)(d-\delta-k_1)(d-\delta-k_1-k_2)(d-c_1)(d-c_1-c_2)\ldots(4),$$
$$=(\phi_2-\phi_1)c_1(c_1-\delta)(c_1-\delta-k_1)(c_1-\delta-k_1-k_2)(c_1-d)(-c_2)\ldots(5),$$
$$=(\sigma-\phi_2)(c_1+c_2)(c_1+c_2-\delta)(c_1+c_2-\delta-k_1)$$
$$(c_1+c_2-\delta-k_1-k_2)(c_1+c_2-d)\,c_2\ldots\ldots\ldots\ldots(6).$$

Although near the shore the depth of the ocean must be less than that of the upper layer of the continental crust, we may suppose it possible that in the abysmal regions it may be deeper. On Mr Jukes-Browne's hypothesis it seems reasonable to consider the continental crust near a flat shore, which is the condition our equations contemplate, to consist in the upper portion of the less ancient strata and in the lower of sub-basic plutonic rocks, or of metamorphosed rocks similar to them in density. Should the assumption of the two layers be found unimportant, it will show that there is no such marked difference in density between the upper and lower portions of the continental crust as to render their attractive influence per unit volume decidedly different, or else that the upper layer is so thin as not to affect it materially.

If we put $\delta = c_1 + h$, this will meet the two possible cases of the ocean being either more or less deep than the upper layer of the continental crust, according as we make h positive or negative. Then

$$(\mu-\rho_1)(c_1+h)(-k_1)(-k_1-k_2)(c_1+h-d)\,h\,(h-c_2)\ldots\ldots\ldots\ldots(1),$$
$$=(\rho_1-\rho_2)(c_1+h+k_1)k_1(-k_2)(c_1+h+k_1-d)(h+k_1)(h+k_1-c_2)\ldots(2),$$
$$=(\rho_2-\sigma')(c_1+h+k_1+k_2)(k_1+k_2)\,k_2\,(c_1+h+k_1+k_2-d)$$
$$(h+k_1+k_2)(h+k_1+k_2-c_2)\ \ldots\ldots\ldots\ldots(3),$$
$$=(\sigma-\sigma')\,d\,(d-c_1-h)(d-c_1-h-k_1)(d-c_1-h-k_1-k_2)$$
$$(d-c_1)(d-c_1-c_2)\ \ldots\ldots\ldots\ldots(4),$$
$$=(\phi_2-\phi_1)\,c_1(-h)(-h-k_1)(-h-k_1-k_2)(c_1-d)(-c_2)\ \ldots(5),$$
$$=(\sigma-\phi_2)(c_1+c_2)(-h+c_2)(-h+c_2-k_1)$$
$$(-h+c_2-k_1-k_2)(c_1+c_2-d)\,c_2\ \ldots\ldots\ldots(6).$$

(1), (5), and (6) give, upon changing the signs of an odd number of factors in each,

$$(\rho_1-\mu)(c_1+h)\,k_1(k_1+k_2)(d-c_1-h)\,h\,(c_2-h)$$
$$=(\phi_2-\phi_1)\,c_1h\,(k_1+h)(k_1+k_2+h)(d-c_1)\,c_2$$
$$=(\sigma-\phi_2)(c_1+c_2)(c_2-h)(k_1-c_2+h)(k_1+k_2-c_2+h)(d-c_1-c_2)c_2,$$

which may be written,

$$\alpha k_1(k_1+k_2)=\beta(k_1+h)(k_1+k_2+h)$$
$$=\beta\{k_1(k_1+k_2)+h(k_1+k_2)+hk_1+h^2\}$$
$$=\gamma(k_1-g)(k_1+k_2-g)$$
$$=\gamma\{k_1(k_1+k_2)-g(k_1+k_2)-gk_1+g^2\}$$
$$\therefore\quad (\alpha-\beta)k_1(k_1+k_2)=\beta\{h^2+h(k_1+k_2)+hk_1\}\ \ldots\ldots\ldots\text{(A)},$$

and $$(\alpha-\gamma)k_1(k_1+k_2)=\gamma\{g^2-g(k_1+k_2)-gk_1\}$$

$$\therefore\quad \frac{\alpha-\beta}{\alpha-\gamma}=\frac{\beta}{\gamma}\,\frac{h^2+h(k_1+k_2)+hk_1}{g^2-g(k_1+k_2)-gk_1};$$

whence $$k_1+k_2=-k_1+f,$$

where $$f=\frac{(\alpha-\beta)g^2-(\alpha-\gamma)\dfrac{\beta}{\gamma}h^2}{(\alpha-\beta)g+(\alpha-\gamma)\dfrac{\beta}{\gamma}h}.$$

Substituting $-k_1+f$ for k_1+k_2 in (A) and solving the quadratic we obtain,

$$k_1=\tfrac{1}{2}f\pm\left\{\tfrac{1}{4}f^2-\frac{\beta}{\alpha-\beta}(h^2+hf)\right\}^{\frac{1}{2}},$$

and since $$k_1+k_2=f-k_1,$$

$$\therefore\quad k_1+k_2=\tfrac{1}{2}f\mp\left\{\tfrac{1}{4}f^2-\frac{\beta}{\alpha-\beta}(h^2+hf)\right\}^{\frac{1}{2}}$$

$$\therefore\quad k_2=\mp 2\left\{\tfrac{1}{4}f^2-\frac{\beta}{\alpha-\beta}(h^2+hf)\right\}^{\frac{1}{2}}.$$

It is evident that the lower sign must be used, and also that, since k_1 is positive,

$$\tfrac{1}{4}f^2>\tfrac{1}{4}f^2-\frac{\beta}{\alpha-\beta}(h^2+hf).$$

$$\therefore\quad \frac{\beta}{\alpha-\beta}h(h+f)>0.$$

Since $f=2k_1+k_2$, and $\delta=c_1+h$, $\therefore\ h=\delta-c_1$, and $h+f$ becomes $2k_1+k_2+\delta-c_1$; and this cannot be negative unless the thickness of the upper layer (c_1) exceeds the whole thickness of the suboceanic crust increased by the addition of the depth of the ocean and the duplication of its upper layer. Upon our

hypothesis this is highly improbable from a geological point of view.

Since therefore we must have $h+f$ always positive, it follows that $\beta/(\alpha-\beta)$ must have the same sign as h.

Now

$$\frac{\beta}{\alpha-\beta}=\frac{(\phi_2-\phi_1)\,c_1h\,(d-c_1)\,c_2}{(\rho_1-\mu)\,\delta\,(d-\delta)\,h\,(c_1+c_2-\delta)-(\phi_2-\phi_1)\,c_1h\,(d-c_1)\,c_2},$$

$$=\frac{(\phi_2-\phi_1)\,c_1\,(d-c_1)\,c_2}{(\rho_1-\mu)\,\delta\,(d-\delta)\,(c_1+c_2-\delta)-(\phi_2-\phi_1)\,c_1\,(d-c_1)\,c_2}.$$

Since we have assumed that ϕ_2 is greater than ϕ_1, the numerator of this is positive, and the sign of the expression will depend upon that of the denominator. Hence, if h can change its sign, the denominator must change sign with it. Should h be anywhere zero, at that place the depth of the ocean becomes equal to the thickness of the upper layer of the continental crust, that is δ becomes c_1, and with this condition the denominator should vanish; or

$$\frac{\rho_1-\mu}{\phi_2-\phi_1}\,c_1\,(d-c_1)\,c_2=c_1\,(d-c_1)\,c_2.$$

Hence we must have either $c_1=0$, or $c_2=0$ because

$$(\rho_1-\mu)/(\phi_2-\phi_1)$$

is not unity. But either of these conditions would reduce the continental crust to a single layer.

Now ρ_1 being the density of some kind of rock and μ of water, we may feel sure that $\rho_1-\mu$ is as large as 1·5. If then we assume roughly 3 for the density of the substratum under the land, and 2·6 for ϕ_1 that of the upper layer c_1, we may fairly put $\sigma-\phi_2=0{\cdot}2$ and $\phi_2-\phi_1=0{\cdot}2$ and then

$$\frac{\rho_1-\mu}{\phi_2-\phi_1}=\frac{1{\cdot}5}{0{\cdot}2}=8, \text{ or rather less.}$$

Hence, seeing that h must have the same sign as $\beta/(\alpha-\beta)$, it must have the same sign as

$$8\delta\,(d-\delta)\,(c_2+c_1-\delta)-c_1\,(d-c_1)\,c_2;$$

or putting $c_1+c_2=25$ miles, h must have the same sign as

$$8\delta\,(d-\delta)\,(25-\delta)-c_1\,(25-c_1)\,(d-c_1).$$

If for any assumed value of δ this is positive when c_1 has that value which makes the second term a maximum, it must be positive for any other value of c_1.

That value of c_1 is $= \frac{25+d}{3} - \left\{\frac{(25+d)^2}{9} - \frac{25d}{3}\right\}^{\frac{1}{2}}$.

Let $d = 100$ miles.

This value is then 11·62.

Let $d = 1000$ miles, it becomes 12·6.

If we then give δ the value one mile, we find that, if c_1 is even as great as 11·62, d being 100 miles, h will have the same sign as 19008 − 13741, and will therefore be positive. It will therefore be positive for every possible value of c_1; and this shows that the upper layer of the continental crust cannot be so thick as one mile.

Assume it however to be one mile thick, and put δ = half a mile. Then h has the same sign as 9751 − 2376, and is still positive; and will therefore be positive for smaller values of c_1. Consequently the thickness of the upper layer cannot be so great as half a mile. The conclusion is similar if we make d larger. Nevertheless when δ is near zero h will become negative, and therefore changes sign somewhere where the ocean is shallow; so that, if we insist upon the typical character of the suboceanic crust extending to the seaboard, this would show that there can be only a single layer in the continental crust. But since near the shore the submarine crust is certainly subject to varying conditions, this is probably not the case; so that the conclusion to be drawn from this enquiry seems to be that, if there are two layers of different densities in the continental crust, the upper one must be so thin as practically to reduce the case to that which we have already examined, in which one layer only has been taken account of.

The above investigation of Mr Jukes-Browne's *a priori* probable hypothesis throws us back in effect upon No. 4, which was superficially discussed in Chapter XVII. We now see that it is the only combination of layers in the crust arranged one and one, one and two, or two and two, beneath the seaboard and the ocean, which is suitable. Of course our result does not strictly

imply that there exists only a single layer in the crust beneath the seaboard, and that one of uniform density, and only two layers each of uniform density beneath any given region of the ocean. But what it does imply is, that the average density of the land crust is practically uniform throughout, and that the average density throughout a small thickness of the upper portion of the suboceanic crust is greater than it, and, contrary to what was assumed in the "summary," greater also than the average density of the much thicker lower portion of the suboceanic crust.

The fuller examination of this 4th hypothesis also confirms the conclusion that the density of the substratum is less beneath the ocean than beneath the land at the seaboard, and that consequently, if the substratum is liquid, it will be affected by ascending currents beneath oceanic regions. As regards the dynamical relation of the different parts of the earth's crust this point is of the chief importance. It is clear that, where the ascending currents are more powerful, they will tend to lift up the crust; and where they are less so, the support of the crust will be more nearly simply hydrostatical. Should the convection currents under any area be becoming weakened, that would indicate that the substratum was becoming more dense. But the thickness and density of the crust would not change so rapidly by congelation as the density of the substratum would be changed, and therefore the adaptation requisite to keep the mass of the frustrum constant must be supplied by the ocean becoming deeper, and so substituting a greater volume of light matter to compensate for the increased density below. This would lead us to suppose that littoral regions subject to frequent earthquakes may be near areas where the currents are growing weaker, and they would consequently indicate sinking areas beneath the neighbouring ocean.

It may be asked what arrangement of the bedding of the crust would satisfy the requirements with respect to the densities. It seems that alternations of nearly horizontal strata of no very great thickness, provided their mean density was constant throughout moderate thicknesses, would produce sensibly the same attraction at the surface as a single layer of uniform

density. So also would a thickness of highly contorted strata whose average density did not differ greatly among themselves when moderate thicknesses were considered. This latter is probably the condition of the main part of the continental crust, because, wherever the more deeply seated rocks are exposed, they are found much contorted, and the materials of the sedimentary rocks which cover them have been more or less lenticularly deposited, and are probably nowhere very thick in comparison with the total thickness of the crust.

With regard to the suboceanic crust, if, as we have been led to believe from our former reasonings, it has suffered but little compression, the existence of two nearly horizontal layers in it is the more easily conceivable. It is evident from the necessary equality of mass of equal frustra at different places, that its lower layer though less dense than the upper must nevertheless be more dense than the continental crust. The upper layer as already suggested may possibly consist largely of basaltic flows from submarine eruptions, or else, if our speculation on the origin of ocean basins be accepted[1], it may consist partly of the dense magma suddenly cooled, while the lower layer consists of crystalline rocks more slowly formed, from which a portion of the more basic minerals have been excluded in the process of crystallization.

Professor Dana has lately published a paper upon "The Origin of the Deep Troughs of the Oceanic Depression[2]." He considers that they are "part of the system of topography and its grander part;" and that "the system of features of the oceanic area are involved in the more general terrestrial features." He appears to mean, that the suboceanic depressions follow the same trends as those which govern the contour of the more nearly adjoining continental surfaces, their inequalities ranging on the whole in systems parallel to each other. Dana discusses the question whether volcanic agencies are likely to have produced the deeper troughs, and decides in the negative. He comes to a similar conclusion with regard to superficial agencies,

[1] Chap. xxv.

[2] "American Journal of Science." Third Series, vol. xxxvii. p. 192. March 1889. With Bathymetric map.

and adds, "If superficially acting causes are insufficient, we are led to look deeper, to the sources of the earth's energies of development, to which the comprehensive system in its structure and physiognomy points." Dr G. M. Dawson also states that, "between the cretaceous and eocene eras, under the influence of enormous pressure acting from the Pacific side, the nearly horizontal strata which bordered the Gold Ranges on the north-east were folded together[1]."

All these conclusions of the veteran American, and also that of the Canadian geologist, seem to be in accordance with our belief, that the elevation of the continental ranges is due to the action of convection currents rising beneath the oceans, and pressing the crust towards the land surfaces; and it is evident that these will work at a greater advantage, if the suboceanic crust dips less deeply into the substratum than the continental crust, because the *quasi* hollow in its underside will give the currents a better purchase on which to act.

[1] Trans. Roy. Soc. Canada, reported in "Nature," vol. XLII. p. 651, 1890.

CHAPTER XXVII.

THICKNESS OF THE CRUST AND THE WORLD'S AGE.

Determination of the manner of melting, temperature of fusion, and latent heat, of Rowley Rag by Professors Rücker and Roberts-Austen—Mathematical relations between the thickness of the crust, the world's age, and the activity of the substratum—Age of the world if the substratum was inert—Thickness of the crust on the same hypothesis—That hypothesis untenable—Thickness of the crust, and delay of thickening, if the substratum is energetic—Numerical values of the same for assumed ages of the world—Consideration of physical changes in the lower parts of the crust—Results now obtained accord with the "Revelations of the Thermometer."

In the sixth chapter some estimates were made respecting the time which must have been requisite for the crust of the earth, on the hypothesis of a molten substratum, to have attained a thickness of 25 miles. Any such estimate necessarily requires the melting temperature and the latent heat to be known. Mallet had given a value for the melting temperature of slag, but nothing was known about the latent heat of any kind of natural rock. Notwithstanding, for any admissible value it seemed certain that the liquid substratum could not be inert, but must exercise a retarding influence upon the thickening, if the age of the world, since a crust began to be formed, is anything nearly as great as geological phenomena lead us to suppose.

The Author, feeling how important it was that experiments should be made to determine the latent heat of rock, and to obtain a more reliable value for its melting temperature, applied to his friend Professor Judd, who at once referred the question

to Professor Rücker, whereupon that gentleman most kindly undertook in conjunction with Professor Roberts-Austen to institute the needful experiments at the Royal College of Science.

The method used was "the method of mixtures," and the higher temperatures were determined by using a thermo-electric junction. The rock examined was the basalt, or dolerite, of Rowley Regis, a lava flow of the carboniferous period, of density 2·82[1]. It was found to begin to soften at 760° C. (1400° F.), and the fusion appeared complete at 920° C. (1688° F.); but since the fusion was gradual, there could not be said to be a definite melting temperature. This rendered it impossible to notice in what manner the density was affected during the process of fusion. The mean specific heat between 20° C. and 760° C. was found to be 0·22, and between 920° C. and 1200° C. the mean specific heat was 0·32, whereas between 760° and 920° the mean heat absorbed was 0·58 calories *per* degree, which of course included both the heat absorbed in the liquefaction and that expended in raising the temperature. This may be expressed by saying that, if τ be the temperature and c the heat absorbed expressed as a function of the temperature,

$$\int_{760}^{920} c\, d\tau = 0{\cdot}58\,(920^\circ - 760^\circ) = 0{\cdot}58 \times 160^\circ.$$

It follows that if we regard the specific heat employed in raising the temperature (apart from the latent heat absorbed in producing fusion) as the mean between the specific heats before and after fusion, that is as $(0{\cdot}22 + 0{\cdot}32)/2 = 0{\cdot}27$, the mean latent heat will be $0{\cdot}58 - 0{\cdot}27 = 0{\cdot}31$, and the whole heat taken up in fusing unit mass of rock will be $0{\cdot}31 \times 160 = 49{\cdot}60$ times the amount of heat required to raise unit mass of water

[1] The analysis of Rowley Rag gives

Silica	49·86	Magnesia	4·39	Iron peroxide	3·36
Alumina	12·75	Soda	5·23	Manganese	1·33
Lime	8·71	Iron protoxide	11·38	Water, etc.	3·0

Prestwich, *Geology*, Vol. I. p. 41.

The rock is slightly less basic than the slags experimented on by Mallet.

through 1° C.[1] Thus 49·60 may be considered as the quantity expressed by Λ at pages 66 and 71. For water $\Lambda = 79{\cdot}25$, but whereas Λ for water corresponds to the definite temperature 0° C., or 32° F., Λ for the rock examined is as it were spread over a range of 160° C., extending from 760°, when it begins to soften, to 920° when it is quite liquid.

If we take the specific heat of solid rock at 0·22 as above given, we shall have the latent heat measured in degrees Fahrenheit, rock being the standard substance, which is λ of the above cited pages,

$$\lambda = \frac{180}{100 \times 0{\cdot}22} \Lambda = 406^\circ \text{ F.}$$

Since the constant we require ought to express the amount of heat communicated to the lower part of the crust during its passage from a mobile to a solid state, it seems that we are entitled to regard 406° F. as the latent heat required by our problem; and then V, the temperature of fusion, will be 1688° F.

The above value is considerably lower than the temperature 3000° F., which Mallet has given for the fusion of slag from an iron furnace. When it is remembered that basalt is a very fusible rock, there can be little doubt that Mallet's value is too high for slag. Seeing however that we have all along regarded the substratum as having the density of basalt, it seems justifiable to make use of the newly determined temperature, recollecting that allowance ought to be made for the probability that, in so doing, we are taking it as low as possible; and still further, that it may be perhaps raised higher by the pressure of the superincumbent crust, although this is by no means certain, the change of density accompanying fusion being unknown.

[1] The above passage, when in MS., was seen and revised by Professor Rücker. An account of these experiments was subsequently given by him at the meeting of the British Association at Cardiff, 1891, of which the following report appeared in "Nature" of September 10 (vol. XLIV. p. 456). "The specific heat increases regularly up to the melting-point, which is not very definite. About this point there is considerable absorption of latent heat. The mean specific heat between 20° and 470° was found to be ·199; between 470° and 750°, ·244; between 750° and 880°, ·626; and between 880° and 1190°, ·323."

We are now in a better position than when Chapter VI. was printed, to apply the equations then obtained to find a relation between the thickness of the crust, the world's age, and the activity of the substratum.

Using the same symbols as before;

t = the number of years since a crust began to be formed.

m = the same in millions of years.

V = the melting temperature, now taken at 1688° F.

κ = 400, the conductivity of rock as defined at page 66.

λ = the latent heat of rock measured by the same units as κ, and now taken at 406° F.

k = the thickness of the crust at the time t.

$k+y$ = the thickness the crust would have attained at the time t, if the substratum had been inert.

$$M=\int_0^\mu \epsilon^{-z}\,dz.$$

The equation to the temperature curve was found to be

$$v=\frac{B}{\sqrt{4\kappa t}}\int_0^x \epsilon^{-\frac{x^2}{4\kappa t}}\,dx,$$

where
$$B=2\mu\epsilon^{\mu^2}\lambda\left(1+\frac{y}{k}\right),$$

μ being assumed a constant such that

$$k=\mu\sqrt{4\kappa t}.$$

It was proved at page 68 that, if the substratum is inert, that is if y is zero, k must have this value, μ being necessarily a constant. But if y is not zero, the constancy of μ is an assumption which the analysis obliges us to make. We saw at page 74 that such an assumption leads to the result that dk/dt, the rate at which the crust actually thickens, and the rate dy/dt at which it is prevented from thickening by the action of the substratum, will both of them vary as $1/\sqrt{t}$, and that their ratio to each other will therefore be constant. It was also pointed out that this is consistent with the probable conditions, since it would make each kind of action more energetic at the commencement. Suppose however, as alternative hypotheses, that the ratio of

dk/dt to dy/dt varies either directly or inversely as some power of t. If directly, then when t is very small the thickening will go on very slowly, and the remelting very rapidly. Now although the latter might be admitted, at the same time the thinness of the crust allowing of a more rapid escape of heat would lead us to expect that the thickening would not be very slow at first. On the other hand, when t became large, we should have the thickening very rapid and the melting off very slow. The latter might be admissible, but, on account of the then thickness of the crust, the loss of heat would be slow, so that the thickening would not be very rapid, because the latent heat could escape only slowly.

If we make the other supposition that the ratio of dk/dt to dy/dt varies inversely as some power of t, then, when t is small, the remelting would be small, and when t became large it would be very rapid, both which conclusions are improbable. The hypothesis that the ratio of dk/dt to dy/dt is constant, and consequently μ constant, is not open to these objections.

We observe that $\frac{dy}{dt}dt + \frac{dk}{dt}dt$ is the thickness of rock of which the latent heat passes into the crust in the interval dt, while of these only $\frac{dk}{dt}dt$ contributes to the thickening of the crust, $\frac{dy}{dt}dt$ being as it were remelted. It is evident that $\frac{dy}{dt}dt$ may be either greater or less than $\frac{dk}{dt}dt$; indeed algebraically it may have any value between zero and infinity.

We have the four data, (1) the temperature V at the bottom of the crust, (2) the gradient β at the surface, (3) the flow of heat depending on the latent heat at the bottom of the crust, and (4) the relation $k = \mu\sqrt{4\kappa t}$. Of these (3) has been already used in determining the constant B. There remain therefore three data available, which give three equations, and there are four unknown quantities, k, t, μ, y/k. Hence one of them must be assumed. If however the substratum is inert, y/k has no existence, and the unknown quantities are only three, k, t, μ; so that they can be all determined.

In fact any fourth relation, to give a fourth equation, would have to be derived from the action of the substratum, the law of which is unknown.

Our three equations are,

$$V = 2\mu\epsilon^{\mu^2} M\lambda \left(1 + \frac{y}{k}\right) \ldots\ldots\ldots\ldots\ldots (1),$$

$$\beta = \frac{V}{M} \frac{1}{\sqrt{4\kappa t}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2),$$

$$k = \mu\sqrt{4\kappa t} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3).$$

First of all let us suppose the substratum to be inert. Then y/k disappears, and the equations become,

$$V = 2\mu\epsilon^{\mu^2} M\lambda \ldots\ldots\ldots\ldots\ldots\ldots\ldots (\mathrm{I}),$$

$$\beta = \frac{V}{M} \frac{1}{\sqrt{4\kappa t}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (\mathrm{II}),$$

$$k = \mu\sqrt{4\kappa t} \ldots\ldots\ldots\ldots\ldots\ldots\ldots (\mathrm{III}).$$

To find μ and its derived function M from (I) put $\mu = 1$. We then find that $2\mu\epsilon^{\mu^2} M \times 406$ becomes $1648{\cdot}4$, whereas $V = 1688$. It appears therefore that μ is near unity. We can now approximate by Taylor's theorem, and we obtain

$$\mu = 1{\cdot}007.$$

When $\mu = 1$, Oppolzer's Table X gives $M = 0{\cdot}7468241$. We can then similarly approximate to the value of M corresponding to $\mu = 1{\cdot}007$ and find it to be

$$M = 0{\cdot}7493736.$$

Substituting these values in the expression for V, they give 1689, the correct value being 1688, or one degree Fah. lower. It is evident that our value for μ is near enough for our purpose. Substituting for β, V, and μ, and dividing by 5280 to reduce feet to miles, (II) gives,

$$k = \frac{51 \times 1688 \times 1{\cdot}007}{5280 \times 0{\cdot}74937},$$

$$= 21{\cdot}91 \text{ miles}.$$

Hence, if the substratum is inert, the new values for the latent heat and melting temperature correspond to a thickness of the crust of about 22 miles.

If we now substitute this value for k in (III), we obtain for the age of the world

$$t = 8{,}248{,}380 \text{ years.}$$

This is a far shorter period than geological phenomena appear to require, for although it is not possible from them to assign any definite superior limit to the world's age, we can form some idea of an inferior limit which it must have exceeded. Sir A. Geikie thinks, that the stratified rocks alone, which contain organic remains, cannot have taken much less than 100 million years for their formation[1]. Hence the conclusion is confirmed, at which we arrived in Chapter VI., using merely tentative values of the melting temperature and latent heat, namely, that the substratum cannot be an inert liquid.

We will now introduce the hypothesis that the substratum is energetic, and delays the freezing of the bottom of the crust. In that case we shall have for our first equation,

$$V = 2\mu\epsilon^{\mu^2} M\lambda \left(1 + \frac{y}{k}\right).$$

Since y/k must be positive, the least value that μ can have, when $V = 1688$ and $\lambda = 406$, is that which we have already found as corresponding to $y/k = 0$, viz. 1·007; and this value must give the largest value possible for k with a given value of β, and we have just seen, if the value of β is taken at 1/51, then that largest value of k will be 21·91 miles, and the corresponding value of t will be 8,248,380 years.

On the other hand, as y/k increases μ will diminish, and, if y/k were to become infinite, μ would be zero, in which case t would be infinite, and the limiting value of k would be given by the relation,

$$k = \frac{V}{\beta}\frac{\mu}{M} \text{ when } \mu = 0.$$

But μ/M diminishes as μ diminishes, and becomes unity when $\mu = 0$.

Hence the least value which k can have is V/β, or

$$51 \times 1688 \text{ feet} = 16{\cdot}30 \text{ miles.}$$

[1] "Text Book of Geology," p. 55, 1882.

We see then that, with the present accepted value of the temperature gradient at the surface and Professor Rücker's values for the complete melting temperature and latent heat of Rowley rag, the possible values for the thickness of the crust range between 16·30 and 21·91 miles, the corresponding time ranging from something over 8 millions of years to infinity. We are tied to these estimates by the assumed values of V, λ, and β; but, if the melting temperature is greater than 1688° F., the values of k will be larger, and it will be recollected that we assumed it formerly, judging from Mallet's experiments, to be 2550° F. It was this which enabled us to assume a thickness of 25 miles for the crust: but if we give it so great a thickness with the present values of V and λ, we shall find that it makes y/k negative, which it cannot be.

We may now make some assumptions and note the consequences. There seems no particular reason to assume one value rather than another for the thickness of the crust between the possible limits, nor yet for the ratio y/k. It will therefore be best to assume values for the age of the world.

We have, $$M = \frac{V}{\beta}\frac{1}{\sqrt{4\kappa t}} = \frac{51 \times 1688}{40 \times 10^3\sqrt{m}},$$

where m is the number of millions of years.

$$\therefore \quad M = \frac{2·1522}{\sqrt{m}}.$$

Suppose the time to have been 100 million years.

Then $$M = 0·21522,$$

and $$\mu = 0·22.$$

But $$k = \frac{V}{\beta}\frac{\mu}{M} = 16·30\frac{\mu}{M} \text{ miles.}$$

$$\therefore \quad k = 16·66 \text{ miles.}$$

And $$1 + \frac{y}{k} = \frac{V}{2\mu\epsilon^{\mu^2}M\lambda},$$

whence $$\frac{y}{k} = 40·83.$$

Next let us suppose the time to have been 50 million years.

Then we find $M = 0{\cdot}30$, and the corresponding value of μ is $0{\cdot}31$.

This gives $k = 16{\cdot}84$ miles,

and $$\frac{y}{k} = 19{\cdot}013.$$

It appears therefore that, if the time is as long as is probable, the present thickness of the crust cannot differ much from 17 miles. But it must not be supposed that it would in the future remain stationary at that value, because, as time went on, β would diminish, and simultaneously with it k would increase.

The reason, why with a given value of the surface gradient β the value of k differs so little for different values of the time when that is large, is, because in that case μ is small, and μ/M is near unity, so that k has nearly the constant value V/β, which is that which it would have on the supposition that the temperature curve is a straight line. This is the way in which the thickness of the crust has been usually computed in geological works.

Our present investigation however gives us the additional information respecting the activity of the substratum in retarding the thickening of the crust. The degree of this activity may be roughly estimated; for since

$$k = \mu\sqrt{4\kappa t},$$

$$\therefore \quad \frac{dk}{dt} = \mu\sqrt{\frac{\kappa}{t}}.$$

If $t = 100$ million years, $\mu = 0{\cdot}22$, and then

$$dk = 0{\cdot}0004\, dt.$$

Hence the present rate of thickening of the crust would be $0{\cdot}0004$ foot per annum, or at the rate, if it continued unchanged, of four feet in 10,000 years.

Since in that case $dy/dt = 40{\cdot}83\, dt$, the rate at which the thickening would be retarded would be

$$dy = 0{\cdot}0179\, dt,$$

or about eighteen feet in 1000 years. This, when compared with a familiar geological standard, is very considerable, being

more than forty times the estimated present mean denudation of the land, which is about one foot in 2400 years[1].

If $t = 50$ million years, the present rate of thickening would be 0·000877 foot per annum, or about 9 feet in 10,000 years; and the retardation of thickening 0·0166 foot per annum, or over 16 feet in 1000 years.

The results may be given as in the annexed table.

World's age in millions of years.	Present thickness of the crust in miles.	Value of the ratio y/k or γ.	Present rate of thickening per 1000 years in feet.	Rate of retardation of thickening per 1000 years in feet.
8	21·91	0		
50	16·84	19·01	0·9	16·6
100	16·66	40·83	0·4	17·9
∞	16·30	∞		

The above calculations have been made as if the substratum yielded up the whole of the latent heat at the bottom of the crust, passing *per saltum* into the solid state. Since this is not what occurs, we must make some allowance for the viscous layer. Above the depth where softening begins the crust may be treated as solid, and if the same temperature curve be carried down from there to the depth where its ordinate indicates the temperature of complete fusion, that depth will be the k of our calculations.

But since physical changes take place in the condition of the crust below the depth where softening begins, and the condition of perfect liquidity is reached only gradually, it seems reasonable to suppose that the distance required for that result will be greater than if the change took place all at once. The flow of heat at a given depth will depend upon the latent heat liberated below that depth. Hence $\kappa\, dv/dx$ will be less for a given depth, or value of x, if only a portion of the latent heat has been liberated below that depth than if the whole has. If κ continued constant, dv/dx would then necessarily be diminished

[1] See Mr Davison's correction of Croll's estimate "Geol. Mag.," N. S., Dec. III., vol. VI., p. 409.

for the viscous region, and the crust would be rendered thicker. It is however possible that κ may diminish in such a manner that dv/dx need not diminish. We have therefore no certainty upon the question whether or not, including the softened portion, the crust is, or is not, thicker than it would be if none of it was softened.

Since the softening commences at 1400° F., and the fusion is complete at 1688° F., we may conclude roughly that the thickness of the softened part is about 0·23, or rather less than a quarter, of the whole thickness.

It is to be noticed that, small as these values of the thickness of the crust appear to be, they come near the values obtained from considerations of densities, and of temperature gradients at the sea level and at Mt S. Gothard, and at Mt Cenis, and that the melting temperature obtained by the same means comes fairly near that of the Rowley rag[1]. The values used for obtaining those results were, for the gradient at the sea level 1/51° F. per foot, at S. Gothard 1/102°, and at Mt Cenis 1/100°. It was assumed that the crust, of density 2·68, rests in hydrostatic equilibrium, on a substratum of density 2·96.

The results obtained were from the S. Gothard data,

Thickness of crust 18·3 miles.
Melting temperature 1944° F.

From Mt Cenis,

Thickness of crust 19·8 miles,
Melting temperature 2105° F.

Prestwich says "on geological grounds the solid crust at all events need not have a thickness of even 20 miles[2]."

[1] Chap. XVI.

[2] "On the agency of water in volcanic eruptions," "Proc. Royal Soc.," No. 246, p. 171, 1886.

CHAPTER XXVIII.

ON THE COOLING OF AN EARTH NOT SOLID.

Depth of level of no strain and mean height of elevations previously obtained for the case of the cooling of a solid earth—Many geologists demur to the hypothesis of solidity—convection currents in layers of varying constitution—Mr T. M. Reade's critique—Argument for rigidity from fortnightly tide further considered—The problem stated for the case of a liquid substratum—Average fall of temperature of a mass will give the correct mean contraction—Mallet's experiments on the contraction of slag in fusion—Calculation of depth of level of no strain with a liquid substratum—Calculation of the corresponding mean height of elevations—Numerical results for certain assumed ages of the world—Amount of radial contraction—Mean amount of fall of temperature of the interior.

In the eighth chapter we have discussed the results of the cooling of the earth upon the supposition of its being solid throughout, and an approximate determination has been given of the depth of the level, above which there would have been compression and below it extension of the materials composing the crust. It appears that such a level of no strain would in that case be met with at the present time at the depth of from one to two miles, or even less, according to what we assume the temperature of solidification to have been. The average height of all the elevations was also calculated, such as could be formed out of the rock situated above the level of no strain, and therefore subjected to compression; and it was found on the most liberal computation scarcely to exceed six feet; that is to say, if all the inequalities so formed were to be levelled down, they would form a coating over the whole globe not exceeding six feet in depth, but probably much less.

Many geologists however do not believe that the earth is

solid throughout[1], and in the preceding chapters we have been led to the view on geological grounds that there is a solid crust resting on a molten substratum of unknown depth; and that this is affected by convection currents; because, if it were not so, the age of the world must be much less than geological considerations lead us to think it to be[2]. It might be objected to this hypothesis that the increase of density downwards in the interior would prevent the play of convection currents. No doubt that would be so where the change of density was due to a change of material, as for instance from molten rock to molten metal. But an increase of density due to pressure only would not have that effect; for as soon as a quantity of liquid began to move to a different level, it would partake of the change of density due to the change of pressure there, and begin to behave like the matter amidst which it had been introduced. It seems then that systems of currents must exist each being confined to a certain thickness of strata of such constituents as could mix by diffusion, the transference of heat from one such layer to another with which it could not mix taking place by conduction across their common boundary.

In order to form an idea of the nature of the matter of the interior which would cause the density to vary according to Laplace's law, let us suppose a globe unaffected by gravity composed of shells of varying compressibility, but of density everywhere the same as that of surface rock; and then suppose gravity to act upon it, and to reduce it to the size and density of the existing earth. This will give an idea of the law of compressibility of matter of surface density at various depths, which would be suitable to sustain the pressure, so as to bring the law of density into accordance with Laplace's law.

[1] Nor does this opinion appear to be confined to geologists. "Notwithstanding the difficulties which arise in connection with the rigidity of the earth under the action of the forces which generate precession, nutation, and the tides, the theory of a comparatively thin crust resting in approximate hydrostatic equilibrium upon a denser substratum is favored by enough facts to render it very plausible." Professsor Harkness "on the Solar Parallax and its related constants including the Figure and Density of the Earth," p. 143, Washington Government Printing-Office, 1891.

[2] p. 23, App.

z = the depth.

ζ = the thickness of rock of surface density which by the compression would be reduced to z.

ρ = the density at z.

s = the surface density, 2·75.

c = the mean coefficient of compressibility at z.

p = the pressure at z.

Then the compression produced upon the element $d\zeta$ to reduce it to dz will be $cpd\zeta$.

$$\therefore\ dz = d\zeta - cpd\zeta.$$

$$\therefore\ \frac{dz}{d\zeta} = 1 - cp.$$

But we have supposed that when the element was of the length $d\zeta$ its density was the surface density s, and when it is reduced to dz it has the density ρ; and the volumes are inversely as the densities.

$$\therefore\ \frac{dz}{d\zeta} = \frac{s}{\rho} = 1 - cp.$$

$$\therefore\ c = \frac{1 - \frac{s}{\rho}}{p}.$$

Expressions for ρ and p are given at p. 29.

Hence the requisite compressibility at any given depth can be found.

Suppose the pressure to be measured in atmospheres. Then c is the compressibility of the matter at the depth z under the pressure of one atmosphere.

It is proved p. 29 that

$$p = \frac{gsA}{2}\left(\frac{\sin^2 qr}{r^2} - \frac{\sin^2 qa}{a^2}\right),$$

$$= \frac{gsA}{2}\left(\frac{\rho^2}{Q^2} - \frac{s^2}{Q^2}\right).$$

This determines the pressure in terms of the density.

And $$s = Q\frac{\sin qa}{a}.$$

$$\therefore\ p = \frac{gsA}{2}\frac{1}{s^2}\frac{\sin^2 qa}{a^2}(\rho^2 - s^2),$$

$$= \frac{gs}{2s^2} \times \frac{a^3}{\sin^2 qa - qa \sin qa \cos qa} \times \frac{\sin^2 qa}{a^2}(\rho^2 - s^2),$$

$$= \frac{gs}{2s^2}\frac{a}{1 - qa \cot qa}(\rho^2 - s^2).$$

$$\therefore\ c = \frac{\rho - s}{\rho}\frac{2s^2(1 - qa\cot qa)}{gsa(\rho^2 - s^2)},$$

$$= \frac{1}{\rho(\rho + s)}\frac{2s^2(1 - qa\cot qa)}{gsa}.$$

This gives the compressibility at the depth where the density is ρ.

If we express gsa in atmospheres it will give the corresponding compressibility.

Now $$qa = 2{\cdot}4605 \text{ in radians},$$
$$= 140^\circ.\ 58'.\ 3''.$$
$$\therefore\ \tan qa = -\,0{\cdot}8105;$$
$$\therefore\ 1 - qa \cot qa = \frac{3{\cdot}2710}{0{\cdot}8105}.$$

Also gs = the weight of a cubic foot of surface rock = 171·875 lbs.

And $$a = 20902404 \text{ feet}.$$

The pressure of the atmosphere = 14·7 lb. per square inch, or 14·7 × 144 lb. per sq. foot.

$$\therefore\ gsa = \frac{171{\cdot}875 \times 20902404}{14{\cdot}7 \times 144} \text{ atmospheres per sq. foot},$$

whence $$\frac{2s^2(1 - qa\cot qa)}{gsa} = 0{\cdot}000035966.$$

At the surface $$\frac{1}{\rho(\rho + s)} = \frac{1}{2s^2},$$

whence the compressibility per atmosphere of surface rock should be

$$0{\cdot}0000023779.$$

In order to obtain the corresponding compressibility at any given depth, we have merely to multiply 0·000035966 by the value of $1/\rho(\rho + s)$ at that depth.

Referring to the table of densities at p. 35, we see that the density at 0·9 of the radius measured from the centre, that is at a depth of 400 miles, is according to Laplace's law 3·88. Whence at 400 miles the compressibility would be 0·000001398.

The compression may be easily found from the densities. It is $(\rho - s)/\rho$; and at 400 miles would be 0·29123.

Now seeing that we have concluded that the density of the substratum at the depth of 25 miles, or, if we accept the results of the preceding chapter, considerably less, is as great as 2·96, while a compressibility so small as 0·000001398[1] would suffice to reduce matter originally of the surface density 2·75 to the density which Laplace's law gives at 400 miles; and that with the not very enormous condensation of 0·29123; it seems not improbable that the substratum may consist of a magma of a uniform composition, at least to the depth of 400 miles, and that whatever increase of density there may be in the magma within that depth may be due to condensation. But when we reach great depths, the appropriate compressibility becomes much smaller, and the condensation becomes very great. At the centre the compressibility would be 0·00000025, and the condensation 0·744; and it is inconceivable that any substance of the nature of fused rock can exist, which could be compressed to less than 3/10ths of its original volume, even under most enormous pressure. This confirms the supposition that the increase of density in the deeper strata is due to the intrinsic density of the materials, and not to condensation[2].

The above considerations agree very well with the hypothesis concerning the relation of the substratum to the nucleus, be it solid or liquid, which we have made in the preceding chapters.

The conditions of the problem of the contraction of the globe cooling under these circumstances will be greatly altered, for our former calculations[3] do not directly apply to the case of a liquid interior, as was justly remarked by Mr T. Mellard Reade in a correspondence published in "Research," in which he wrote,

[1] The compressibility of water for one atmosphere is 0·0000478.

[2] See p. 33.

[3] Chap. VIII.

"If we accept the new hypothesis of convection currents, Mr Fisher's careful calculations—which were made on the hypothesis of cooling by conduction, and go to show that the corrugations of the earth's crust could not have been produced by the shrinking of the nucleus—will have to be revised[1]."

It is in accordance with this suggestion that the following supplementary calculations have been undertaken. But, inasmuch as they proceed upon the hypothesis of internal liquidity, we will preface them by stating somewhat more fully than has been done[2] the reasons for believing, that this hypothesis is not in reality excluded by such results of observations as have hitherto been obtained in the search for a measurable fortnightly tide; for it may be conceded that it would be an argument in favour of the doctrine of the solidity of the earth if it could be shown that a fortnightly tide actually exists in nature, agreeing in amount and in the time of its occurrence with what that tide would be when calculated on hydrodynamical principles. But, without insisting on such agreement in the height and time of occurrence, it would still be an argument in favour of solidity, if observations proved that there is such a tide at all. Now it is certain that, if at a given port a tide exists, the average height of that tide ought to be always the same year by year; and its lag ought to be the same. But in fact the annual average of the height of the fortnightly disturbances of the sea at Karachi in India, which is the port where the necessary observations have been carried out for fifteen years, does not maintain anything like a constant value. This appears to show that no conclusion can be drawn from the observations to decide the question whether there is such a tide or not. The disturbances may be wholly due to meteorological causes. At any rate, if such a tide exists, it is so masked by meteorological disturbances as to be unrecognisable. The irregularity of the times of disturbances is equally noticeable with the irregularity of their amount. Both are given in the table below.

[1] See "Research," March and April, 1890.

[2] See p. 41, Chap. III, note, and p. 346. Summary.

Years.	Greatest height of water above the mean in inches.	Lag of the tide in days.
1868–9	0·636	12·36
1869–70	0·936	12·09
1870–1	0·444	10·07
1871–2		
1872–3		
1873–4	0·144	8·67
1874–5	0·456	1·59
1875–6	0·120	0·93
1876–7	0·384	0·04
1877–8	0·564	1·17
1878–9		
1879–80	0·360	12·75
1880–1	0·240	2·87
1881–2	0·408	4·97
1882–3	0·072	12·28

Mean of all the heights 0·396 inch.

We may notice that the least elevation is 0·072 and the greatest 0·936. The difference between these is 0·864, which is more than double the mean of the whole series of observations. This would seem to indicate the existence of a disturbing cause of the same order of magnitude as the quantity to be measured.

To recapitulate what has been previously said[1], the existence of a level of no strain in a solid crust may be thus explained. Fixing the attention upon a particular spherical shell of rock at a given epoch, we find it continuous and without open cracks. Its temperature is falling, and it is consequently shrinking, while at the same time, owing to the contraction of the sphere of matter interior to it, the shell is about to sink into a position where, being nearer to the centre of the sphere, it will find less room to occupy. If then the shrinking of the shell happens to be exactly equal to the loss of room, the shell will not be strained. The place of that particular shell indicates the position of the level of no strain at that precise epoch.

Now in the case where a solid crust is gradually being

[1] Chap. VIII.

formed out of a liquid substratum by congelation of the magma on to the bottom of the crust, it is obvious that each layer as it solidifies will be of the exact size of the surface of the substratum at the time. Consequently the bottom of the crust will not be itself strained. And since the temperature at that level will always be the highest at which the matter there can exist in a solid state, it follows that every layer above the bottom will be cooling and contracting. Whether by that means a level of no strain will be produced between the bottom of the crust and its upper surface is a question which can only be settled by calculation, taking into account the probable circumstances of the case.

To form a clear conception of our problem we must bear in mind, that we are applying the mathematical principles of conduction of heat in one dimension in a solid, one fixed plane of which (the upper) is supposed to be kept always at a constant temperature, which we take for the zero of our scale, while another plane, parallel to it, is maintained at a certain temperature V, the distance between these two planes being always proportional to the square root of the time elapsed since they coincided, $(k = \mu\sqrt{4\kappa t})$[1]. On account of the large ratio which the radius of the earth bears to the thickness (k) of the crust, the principles of the conduction of heat in one dimension are sufficiently applicable in this case.

The question whether there will be a level of no strain within the solid crust depends, as just stated, upon whether the rate, at which some shell is contracting through loss of heat, is the same as the rate, at which the space which it finds to occupy is diminishing, owing to the shell sinking to a lower level through the contraction of all the matter interior to it. We are treating this question as one depending upon the contraction of a solid mass through cooling arising from the conduction of heat within it. It is clear therefore that a mathematical investigation on these lines will not apply to the bottom layer of the crust, which is under different physical conditions from the other parts of it, because it does not consist of solid matter in the midst of solid matter contracting through cooling, but of a

[1] Pages 73 and 21 App.

layer of liquid continually passing into solid by congelation. It is therefore possible, indeed certain, that the bottom of the crust may not be strained, and nevertheless it may not be what we are mathematically defining as a level of no strain; for that applies only to matter which is already solid at the time under consideration.

The following symbols will be used.

r = the radius of the earth at present, taken at 3959 miles.

t = the time elapsed since a crust began to be formed, the unit of time being one year.

V = the temperature of solidification or incipient fusion of rock.

v = temperature of a spherical shell at the depth x at the time t.

x = the distance from the upper surface of a spherical shell of elementary thickness dx.

κ = the conductivity of the rock composing the crust, as defined at p. 66. Its value is taken as 400.

k = the thickness of the cooled crust at the time t.

E = the coefficient of mean voluminal contraction of rock.

N = the coefficient of mean voluminal contraction of the matter of the interior.

w = the mean temperature of the liquid interior at the time t.

W = the mean temperature of the same at the first.

B = the temperature defined by $B = V\Big/\int_0^\mu \epsilon^{-y^2}dy$.

γ = the ratio of remelting to thickening of the crust.

We will first show that the average fall of temperature of the interior will give the correct amount of its contraction. It will evidently do so if convection continually equalises the temperature throughout. But if this is not the case, suppose the whole mass to be made up of m portions, $a_1, a_2, \ldots\ldots a_m$; and let the change of the amount of heat contained in these respectively during a given interval be $h_1, h_2, \ldots\ldots h_m$. Then their changes of temperature may be taken to be $h_1/a_1, h_2/a_2, \ldots\ldots h_m/a_m$. In the meanwhile the change of volume in each severally will be

$N\frac{h_1}{a_1}a_1$, $N\frac{h_2}{a_2}a_2$, ...$N\frac{h_m}{a_m}a_m$, N being the coefficient of voluminal contraction, and the total change of volume will be the sum of these. At the same time the average fall of temperature of the whole will be $\frac{h_1+h_2+\ldots\ldots+h_m}{a_1+a_2+\ldots\ldots+a_m}$; and the change of volume calculated from this will be in the given interval,

$$N\frac{h_1+h_2+\ldots\ldots+h_m}{a_1+a_2+\ldots\ldots+a_m}\times(a_1+a_2+\ldots\ldots+a_m),$$

which $= N\frac{h_1}{a_1}a_1 + N\frac{h_2}{a_2}a_2 + \ldots\ldots + N\frac{h_m}{a_m}a_m$ as before.

In estimating the change of a magnitude (a) owing to a change of temperature (θ), we may employ either the original magnitude (a) or the altered magnitude (a'), if we neglect powers of the coefficient of contraction above the first; for the change of a is $ae\theta$, and the change of a' is $a'e\theta$, which equals $ae\theta + ae^2\theta^2$, or to the first power of the coefficient of expansion $ae\theta$, as before.

Hence, although r the radius of the sphere at the present time has changed during the interval t owing to changes of temperature, we may nevertheless use the present value of r in terms which involve the coefficient of contraction.

Reverting to the method pursued in the case of a solid earth at p. 95, and making the necessary alterations as we proceed, the volume of the shell of the solidified crust at the depth x will be $4\pi(r-x)^2\,dx$, and this will be changed in the interval dt by

$$E\,4\pi(r-x)^2\,dx\,\frac{dv}{dt}\,dt.$$

The whole change of volume of the solidified crust interior to this in the interval dt will therefore be

$$E\,4\pi\int_x^k(r-x)^2\frac{dv}{dt}\,dx\,dt.$$

Calling w the mean temperature of the liquid interior at the time t, the change of volume of this in the same interval will be

$$N\frac{4}{3}\pi(r-k)^3\frac{dw}{dt}\,dt.$$

Hence Δ, the change of volume of the whole sphere interior to the layer at x, will be the sum of these; so that the volume of sphere interior to x becomes

$$\frac{4}{3}\pi (r-x)^3 + \Delta,$$

or if dz be the change of radius to this shell

$$\frac{4}{3}\pi (r-x)^3 + \Delta = \frac{4}{3}\pi (r-x+dz)^3 = \frac{4}{3}\pi (r-x)^3 + 4\pi (r-x)^2 dz$$

approximately.

Consequently the change in the radius will be

$$\begin{aligned} dz &= \frac{\Delta}{4\pi (r-x)^2} \\ &= \frac{E}{(r-x)^2}\int_x^k (r-x)^2 \frac{dv}{dt}\, dx\, dt + \frac{N}{3}\frac{(r-k)^3}{(r-x)^2}\frac{dw}{dt}\, dt, \end{aligned}$$

while the circumference of the said sphere will in the same interval be altered by the above expression multiplied by 2π.

The circumference of the shell itself, owing to the change of temperature in the matter of which it is composed, will in the same interval be altered by

$$2\pi \frac{E}{3}(r-x)\frac{dv}{dt}\, dt.$$

Hence, seeing that $\frac{dv}{dt}$ and $\frac{dw}{dt}$ are both clearly negative ratios we obtain the criterion that,

as $$\frac{1}{(r-x)^2}\int_x^k (r-x)^2 \frac{dv}{dt}\, dx + \frac{N}{3E}\frac{(r-k)^3}{(r-x)^2}\frac{dw}{dt} \lesseqgtr \frac{1}{3}(r-x)\frac{dv}{dt},$$

so will the shell at the depth x be compressed, not strained, or extended.

For $\frac{dv}{dt}$ write its equivalent $\kappa \frac{d^2v}{dx^2}$. Then

$$\begin{aligned} \kappa \int (r-x)^2 \frac{d^2v}{dx^2}\, dx &= \kappa \left\{(r-x)^2 \frac{dv}{dx} + 2\int (r-x)\frac{dv}{dx}\, dx\right\} \\ &= \kappa \left\{(r-x)^2 \frac{dv}{dx} + 2rv + 2\int -x\frac{dv}{dx}\, dx\right\}. \end{aligned}$$

But $$\frac{dv}{dx} = \frac{B}{\sqrt{4\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}}\text{[1]},$$

whence $$\int - x \frac{dv}{dx} dx = \frac{B}{\sqrt{4\kappa t}} \frac{4\kappa t}{2} \epsilon^{-\frac{x^2}{4\kappa t}}.$$

Also $$\frac{dv}{dt} = \kappa \frac{d^2 v}{dx^2} = \frac{-\kappa B}{\sqrt{4\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} \frac{2x}{4\kappa t}.$$

The above criterion is therefore equivalent to

$$\frac{\kappa}{(r-x)^2}\left[(r-x)^2 \frac{B}{\sqrt{4\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} + 2rv + \frac{2B}{\sqrt{4\kappa t}} \frac{4\kappa t}{2} \epsilon^{-\frac{x^2}{4\kappa t}}\right]_x^k$$
$$+ \frac{N}{3E} \frac{(r-k)^3}{(r-x)^2} \frac{dw}{dt} \lesseqgtr -\frac{1}{3}(r-x) \frac{\kappa B}{\sqrt{4\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} \frac{2x}{4\kappa t}.$$

Now, according to the assumption made in Chapter VI., p. 73,

$$k = \mu\sqrt{4\kappa t}$$

μ being a constant[2].

$$\therefore \quad \frac{1}{\sqrt{4\kappa t}} = \frac{\mu}{k}.$$

Hence between the limits, we have,

$$\frac{\kappa}{(r-x)^2}\left\{(r-k)^2 \frac{B}{\sqrt{4\kappa t}} \epsilon^{-\mu^2} + 2rV + B\sqrt{4\kappa t}\, \epsilon^{-\mu^2} - (r-x)^2 \frac{B}{\sqrt{4\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}}\right.$$
$$\left. - 2rv - B\sqrt{4\kappa t}\, \epsilon^{-\frac{x^2}{4\kappa t}}\right\} + \frac{N}{3E} \frac{(r-k)^3}{(r-x)^2} \frac{dw}{dt}$$
$$\lesseqgtr -\frac{1}{3}(r-x) \frac{\kappa B}{\sqrt{4\kappa t}} \frac{2x}{4\kappa t} \epsilon^{-\frac{x^2}{4\kappa t}}.$$

In order to determine the mean rate of the fall of temperature of the interior expressed by dw/dt, we must consider what takes place at the bottom of the crust in the interval dt.

(1) A layer of thickness dk is frozen on to the underside of the crust, and yields up its latent heat $4\pi(r-k)^2 \lambda dk$, which is conducted upwards into the crust.

(2) A layer of thickness dy is remelted, or rather prevented from freezing, and its latent heat, $4\pi(r-k)^2 \lambda dy$, is thereby

[1] See page 67. [2] See page 21 App.

abstracted from the interior liquid mass, having escaped through the crust. It is this remelting which keeps the surface of the liquid down to the melting temperature V.

The remelted layer will eventually sink by convection, and reduce the mean temperature of the interior mass. The latent heat λ being measured in terms of crust rock as the standard substance, since there is no other mode of escape for the internal heat while the specific heat of fused rock exceeds that of solid by 0·10 only, we shall have nearly for the mean fall of temperature the above amount of heat divided by the volume of the interior, or

$$dw = -\frac{4\pi (r-k)^2 \lambda dy}{\frac{4}{3}\pi (r-k)^3}$$

$$= -\frac{3\lambda dy}{r-k},$$

$$\therefore \frac{dw}{dk} = -\frac{3\lambda}{r-k}\frac{dy}{dk}$$

$$= -\frac{3\lambda}{r-k}\gamma,$$

where, if μ is constant, γ is constant[1],

$$\therefore \frac{dw}{dt} = -\frac{3\lambda\gamma}{r-k}\frac{dk}{dt}$$

$$= -\frac{3\lambda\gamma}{r-k}\mu\sqrt{\frac{\kappa}{t}}$$

$$= -\frac{6\lambda\gamma\mu^2\kappa}{(r-k)\,k}.$$

Substituting for dw/dt and multiplying by $(r-x)^2/\kappa$ our expression becomes

$$(r-k)^2 \frac{B}{\sqrt{4\kappa t}}\epsilon^{-\mu^2} + 2rV + B\sqrt{4\kappa t}\,\epsilon^{-\mu^2} - (r-x)^2\frac{B}{\sqrt{4\kappa t}}\epsilon^{-\frac{x^2}{4\kappa t}}$$

$$- 2rv - B\sqrt{4\kappa t}\,\epsilon^{-\frac{x^2}{4\kappa t}} - 2\frac{N}{E}\frac{(r-k)^2}{k}\lambda\gamma\mu^2$$

$$\lesseqgtr -\frac{1}{3}(r-x)^3\frac{B}{\sqrt{4\kappa t}}\frac{2x}{4\kappa t}\epsilon^{-\frac{x^2}{4\kappa t}}.$$

[1] See page 74.

Divide by $\dfrac{B}{\sqrt{4\kappa t}}$ and put $\sqrt{4\kappa t}=\dfrac{k}{\mu}$.

$$(r-k)^2\epsilon^{-\mu^2}+\frac{2rV}{B}\frac{k}{\mu}+\frac{k^2}{\mu^2}\epsilon^{-\mu^2}-(r-x)^2\epsilon^{-\frac{x^2\mu^2}{k^2}}$$
$$-\frac{2rv}{B}\frac{k}{\mu}-\frac{k^2}{\mu^2}\epsilon^{-\frac{x^2\mu^2}{k^2}}-2\frac{N}{E}(r-k)^2\frac{\lambda\gamma\mu}{B}$$
$$\lesseqgtr -\frac{1}{3}(r-x)^3\,2x\frac{\mu^2}{k^2}\epsilon^{-\frac{x^2\mu^2}{k^2}}.$$

Divide by r^2 and neglect terms in $1/r^2$,

$$\left(1-2\frac{k}{r}\right)\epsilon^{-\mu^2}+\frac{2V}{B}\frac{k}{\mu r}-\left(1-\frac{2x}{r}\right)\epsilon^{-\frac{x^2\mu^2}{k^2}}-\frac{2v}{B}\frac{k}{\mu r}$$
$$-2\frac{N}{E}\left(1-\frac{2k}{r}\right)\frac{\lambda\gamma\mu}{B}$$
$$\lesseqgtr -\frac{1}{3}\left(r-3x+\frac{3x^2}{r}\right)2x\frac{\mu^2}{k^2}\epsilon^{-\frac{x^2\mu^2}{k^2}}.$$

Collect the terms and multiply by $\epsilon^{\frac{x^2\mu^2}{k^2}}$.

$$\left(1-2\frac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\frac{N}{E}\frac{\lambda\gamma\mu}{B}\right)\epsilon^{\frac{x^2\mu^2}{k^2}}+\left(\frac{2V}{B}-\frac{2v}{B}\right)\frac{k}{\mu r}\epsilon^{\frac{x^2\mu^2}{k^2}}-\left(1-\frac{2x}{r}\right)$$
$$\lesseqgtr -\frac{1}{3}\left(r-3x+\frac{3x^2}{r}\right)2x\frac{\mu^2}{k^2}.$$

Now $\dfrac{V}{B}=M$; and $v=\dfrac{B}{\sqrt{4\kappa t}}\displaystyle\int_0^x\epsilon^{-\frac{x^2}{4\kappa t}}dx,$

$$=\frac{B\mu}{k}\int_0^x\left(1-\frac{x^2\mu^2}{k^2}+\frac{1}{2}\frac{x^4\mu^4}{k^4}-\&c.\right)dx.$$

$$\therefore\quad \frac{2v}{B}\frac{k}{\mu r}=\frac{2}{r}\left(x-\frac{1}{3}\frac{x^3\mu^2}{k^2}+\frac{1}{10}\frac{x^5\mu^4}{k^4}-\&c.\right).$$

Whence $\left\{\left(1-2\dfrac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\dfrac{N}{E}\dfrac{\lambda\gamma\mu}{B}\right)+\dfrac{2M}{\mu}\dfrac{k}{r}\right.$

$$\left.-\frac{2x}{r}\left(1-\frac{1}{3}\frac{x^2\mu^2}{k^2}+\frac{1}{10}\frac{x^4\mu^4}{k^4}-\right)\right\}\left\{1+\frac{x^2\mu^2}{k^2}+\frac{1}{2}\frac{x^4\mu^4}{k^4}+\right\}-\left(1-\frac{2x}{r}\right)$$
$$\lesseqgtr -\frac{1}{3}(r-3x)\,2x\frac{\mu^2}{k^2}-\frac{2x}{r}\frac{x^2\mu^2}{k^2}.$$

We have now to determine which terms may be neglected.

We must in the first place decide what value to assign to the ratio N/E between the coefficients of contraction of the interior and of the crust. We have however no means of knowing with certainty the coefficient of contraction of the spherical nucleus be it solid or liquid which we suppose to underlie the substratum. This may consist of materials which cannot mix with or become dissolved in fused rock and are intrinsically of a higher specific gravity. Nevertheless it is not probable that the mean coefficient of contraction for them differs very greatly from that of fused rock. Mallet, in his experiments made on a large scale upon the contraction of slags[1], endeavoured to obtain the rates of contraction, during the time they remained liquid after being poured into his moulds, as well as during the time of cooling subsequent to their solidification. Although Mallet does not claim exactness for his results, which it is evident he could not obtain owing to the difficulty of fixing the temperature of solidification, nevertheless they are probably the best available, and are sufficiently reliable for our purpose.

His experiments give for the rate of contraction per degree Fah., 0·0000471 between 3680° and 3419°[2], and 0·000026 between 3419° and 3000°.[3] The mean for these two intervals comes out 0·000034; with which if we take $E = 0{\cdot}000021$ as we have done hitherto, $N/E = 1{\cdot}6$. But if we take the higher rate, then $N/E = 2{\cdot}2$. We may therefore for simplicity and with a fair amount of probability assume $N/E = 2$.

Let us then examine the state of things at the depth of five miles when 100 millions of years have elapsed since a crust began to be formed. We shall have in that case $\mu = 0{\cdot}22$, $x = 5$, $k = 16{\cdot}66$ and $r = 3959$.

Referring to the table at page 27 App., and taking the value 40·83 for γ or y/k corresponding to the above numbers, if we take $N = 2E$, we find

$$\epsilon^{-\mu^2} - 2\frac{N}{E}\frac{\lambda\gamma\mu}{B} = -0{\cdot}9071845.$$

[1] "Phil. Trans. Roy. Soc." Vol. 163, p. 196.

[2] Mallet says 0·0000477, but this seems to be a misprint.

[3] "Addition to paper on volcanic energy," "Phil. Trans." Vol. 163, pt. 1.

Our expression then gives

$$-1\cdot 8952 \lesseqgtr -2\cdot 2925.$$

Noticing the signs it appears that the left-hand member is the larger. Consequently there will in this case be stretching at the depth of five miles. But if we put $x=0$, the left-hand member becomes the smaller, which shows that there will be compression at the surface. We may therefore conclude that a level of no strain lies between the surface and a depth of five miles. Seeing then that x cannot be so great as 5, we need not retain any terms in $x^4\mu^4/k^4$ nor yet terms in $x^3\mu^2/rk^2$. Our equation for finding the depth of the level of no strain will then be reduced to

$$\left\{\left(1-2\frac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\frac{N}{E}\frac{\lambda\gamma\mu}{B}\right)+2\frac{M}{\mu}\frac{k}{r}\right\}\left(1+\frac{x^2\mu^2}{k^2}\right)-1$$
$$=-\frac{1}{3}(r-3x)\,2x\frac{\mu^2}{k^2}.$$

$$\therefore\ \left\{\left(1-2\frac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\frac{N}{E}\frac{\lambda\gamma\mu}{B}\right)+2\frac{M}{\mu}\frac{k}{r}-1\right\}\frac{k^2}{\mu^2}$$
$$+\left\{\left(1-2\frac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\frac{N}{E}\frac{\lambda\gamma\mu}{B}\right)+2\frac{M}{\mu}\frac{k}{r}\right\}x^2=-\frac{2}{3}rx+2x^2,$$

whence putting

$$\left(1-2\frac{k}{r}\right)\left(\epsilon^{-\mu^2}-2\frac{N}{E}\frac{\lambda\gamma\mu}{B}\right)=\beta$$

and

$$2\frac{M}{\mu}\frac{k}{r}=\nu,$$

and transposing,

$$(\beta+\nu-1)\frac{k^2}{\mu^2}=(2-\beta-\nu)\,x^2-\frac{2}{3}rx;$$

and therefore

$$x=\frac{r\pm\left(r^2+9\,(\beta+\nu-1)(2-\beta-\nu)\dfrac{k^2}{\mu^2}\right)^{\frac{1}{2}}}{3\,(2-\beta-\nu)},$$

$$=-\frac{3}{2}(\beta+\nu-1)\frac{k^2}{r\mu^2}+\frac{27}{8}(2-\beta-\nu)(\beta+\nu-1)^2\frac{k^4}{r^3\mu^4}-\&c.$$

Neglecting the second term as very small, this gives with

the values for the constants assumed above, writing x_0 for the depth of the level of no strain, since $1-\nu-\beta=1{\cdot}8913$,

$$\therefore \; x_0 = 21697{\cdot}4 \text{ feet} = 4{\cdot}109 \text{ miles.}$$

We have thus found that, if the time elapsed since a crust began to be formed has been 100 million years, the depth of the level of no strain at the present time will be about four miles.

If we take the time as 50 million years, we have $\mu=0{\cdot}31$, $M=0{\cdot}30$, $k=16{\cdot}84$, $\gamma=19{\cdot}013$, and we then find $x_0=2{\cdot}015$ miles, or the depth of the level of no strain is about two miles if the age of the world is only 50 million years.

It appears that, to the degree of approximation which we have adopted, it will be sufficient to retain only the first term in the expression for x_0, and then we have generally,

Depth of level of no strain

$$x_0 = \frac{3}{2}\left\{1 - 2\frac{M}{\mu}\frac{k}{r} + \left(1 - 2\frac{k}{r}\right)\left(2\frac{N}{E}\frac{\lambda\gamma\mu}{B} - \epsilon^{-\mu^2}\right)\right\}\frac{k^2}{r\mu^2}.$$

The fact that a tendency to stretching extends throughout the crust, excepting a comparatively thin layer of it near the surface, must be considered very favourable to the intrusion of molten rock from below, and to all the results which we have attributed to that mode of action of the magma. And since the depth of the level of no strain was much less in early times, this mode of action must have been very effective then. We see also herein a probable explanation of the cause of the deep faults, which have cut through the entire thickness of the crust, and often admitted the intrusion of liquid rock into gaping fissures.

Having found the depth of the level of no strain, we can now estimate the mean height of the elevations of the surface which can have been formed out of the material above that level owing to the shrinking of the globe through cooling.

The calculation will follow to the middle of page 101 that already given in Chapter VIII. for the case of a solid globe, if we merely use B in the place of b, and retain $\sqrt{4\kappa t}$ in those places

where we have used a instead of it. We have there neglected a term in $(\sqrt{4\kappa t}/r)^3$. In the present case we shall have

$$\left(\frac{\sqrt{4\kappa t}}{r}\right)^3 = \left(\frac{k}{\mu r}\right)^3 = 0{\cdot}000007 \text{ (about)},$$

which may still be neglected; and the integration for the time will in the present case also give

$$-\frac{dh}{dx} = 2e\phi - 2e\theta \quad \ldots\ldots\ldots\ldots\ldots\ldots\text{(A)},$$

where, as before, ϕ will have to be obtained by integrating $d\phi/dt$, which expresses the rate at which the temperature of any shell was falling at the moment when the level of no strain was passing through it, and where θ is the temperature of the shell at x at some level above it.

If we write

$$n = 1 - 2\frac{M}{\mu}\frac{k}{r} + \left(1 - 2\frac{k}{r}\right)\left(2\frac{N}{E}\frac{\lambda\gamma\mu}{B} - \epsilon^{-\mu^2}\right),$$

we then have for the depth of the level of no strain,

$$x_0 = \frac{3}{2}n\frac{k^2}{r\mu^2},$$

$$= \frac{3}{2}n\frac{4\kappa t}{r}.$$

We shall have generally for the fall of temperature at a depth x,

$$\theta = A - \frac{B}{\sqrt{4\kappa t}}x.$$

$$\therefore \quad \frac{d\theta}{dt} = \frac{1}{2}\frac{B}{\sqrt{4\kappa}}t^{-\frac{3}{2}}x.$$

This expresses the time rate at which the temperature of a shell at any depth is falling at the time t. Consequently if at that moment the level of no strain happens to be passing through that shell, the rate at which its temperature is then falling will be given by assigning to x the value corresponding to the depth of no strain at that time, which is $\frac{3}{2}n\frac{4\kappa t}{r}$; and the rate so obtained will be $\frac{d\phi}{dt}$.

$$\therefore \quad \frac{d\phi}{dt} = \frac{1}{2}\frac{B}{\sqrt{4\kappa}} t^{-\frac{3}{2}} \times \frac{3}{2} n \frac{4\kappa t}{r},$$

$$= \frac{3}{2} B \frac{n}{r} \sqrt{\frac{\kappa}{t}},$$

whence
$$\phi = \frac{3}{2} B \frac{n}{r} \sqrt{4\kappa t} + f(x),$$

$$= \frac{3}{2} B \frac{n}{r} \frac{k}{\mu} + f(x),$$

the integral being required between the limits of k corresponding to $t = 0$ and to $t =$ the epoch when the shell in question was a shell of no strain.

Therefore substituting for ϕ and θ in (A)

$$-\frac{dh}{dx} = 2e\left(\frac{3}{2} B \frac{n}{r} \frac{k}{\mu} - A + \frac{B\mu}{k} x\right) + f(x).$$

When the level of no strain is at the shell, dh/dx will be zero; and since x will then be equal to $\frac{3}{2} n \frac{k^2}{r\mu^2}$, $\frac{k}{r\mu}$ will equal $\sqrt{\frac{2}{3}\frac{x}{nr}}$.

$$\therefore \; 0 = 2e\left(\frac{3}{2} Bn \sqrt{\frac{2}{3}\frac{x}{nr}} - A + \frac{B\mu}{k} x\right) + f(x).$$

$$\therefore \; -\frac{dh}{dx} = 3eBn\left(\frac{k}{r\mu} - \sqrt{\frac{2}{3}\frac{x}{nr}}\right),$$

and
$$-h = 3eBn\left(\frac{kx}{r\mu} - \frac{2}{3}\sqrt{\frac{2}{3}\frac{x^3}{nr}}\right) + C.$$

Since no contribution to the surface elevations comes from the level of no strain, this has to be taken from $x = \frac{3}{2} n \frac{k^2}{r\mu^2}$ to 0.

And therefore
$$h = \frac{3}{2} eB \frac{n^2}{\mu^3} \frac{k^3}{r^2},$$

where $B = \frac{V}{M}$, and $n = 1 - 2\frac{M}{\mu}\frac{k}{r} + \left(1 - 2\frac{k}{r}\right)\left(2\frac{N}{E}\frac{\lambda\gamma\mu}{B} - \epsilon^{-\mu^2}\right)$.

If the time is 100 million years $n = 1{\cdot}8913$.

If the time is 50 million years $n = 1{\cdot}8022$.

We have
$$x_0 = \frac{3}{2}\frac{nk^2}{r\mu^2}.$$

Consequently, when we have found the depth of the level of no strain, we can deduce the corresponding mean height of the elevations from it by means of the relation

$$h = \frac{eBn}{\mu}\frac{k}{r}x_0.$$

Thus the depth of the level of no strain corresponding to the assumptions we have made when the time is 100 millions of years being 4·109 miles, the mean height of the elevations of the surface above the datum level will be 43·7 feet.

If the time be taken to have been 50 millions of years it will be 10·36 feet.

These estimates are made, as will be remembered, upon the supposition that the thickness of the crust and the melting temperature are according to the determinations in the preceding chapter[1], the coefficient of contraction of the crust rock 0·0000071 linear, the coefficient of contraction of the internal liquid twice as great. These are Mallet's determinations of the coefficients. If we take a larger value for that of the interior, then the value of h will be larger. For instance, if instead of twice we suppose it three times as great as that of crust rock, then, if the time be 100 millions of years, h will be 97 feet.

These estimates evidently point to the conclusion, that we cannot attribute to compression arising from cooling merely the large amount of elevations which actually exist, any more upon the hypothesis of a liquid than of a solid interior.

We may find the radial contraction from the consideration that at any moment the rate of contraction of the radius to the level of no strain is equal to that of the circumference of that shell.

Hence, if we neglect the contraction of the thin shell above the level of no strain, we shall have the radial contraction

$$\int_0^t er\frac{d\phi}{dt}\,dt = \int_0^t \frac{3}{2}er\frac{Bn}{r}\sqrt{\frac{\kappa}{t}}\,dt,$$

$$= 3eBn\sqrt{\kappa t},$$

which varies nearly as the square root of the time.

[1] See p. 27 App.

If the time is 100 million years and $N = 2E$, the radial contraction $= 12$ miles. If $N = 3E$, it will be 18 miles.

If the time has been 50 million years, and $N = 2E$, the radial contraction will have been 5·7 miles. But, if $N = 3E$, it will have been 8·4 miles.

The expression for dw/dk enables us to find through how many degrees the mean temperature of the interior has fallen; for

$$\frac{dw}{dk} = -\frac{3\lambda}{r-k}\gamma.$$

$$\therefore\ w = 3\lambda\gamma \log_\epsilon (r-k) + C.$$

When k was nothing, w was the initial mean temperature W.

$$\therefore\ W = 3\lambda\gamma \log_\epsilon r + C.$$

$$\therefore\ W - w = -3\lambda\gamma \log_\epsilon \frac{r-k}{r} = 3\lambda\gamma \frac{k}{r} \text{ nearly}$$

is the mean fall of temperature from this cause.

If the time has been 100 millions of years, this shows that the mean temperature of the interior must be nearly as high as it ever was, the mean fall being about 209° F., so that in early geological times the globe as a whole was not very much hotter than it is now. Any more frequent exhibition of volcanic energy, such as there seems to have been in those days, must therefore be attributed to the fiery magma being covered by a thinner crust, and containing more occluded gas, rather than to its being itself at a much higher temperature.

There can be no doubt however that the extravasation of water substance by volcanic action must have somewhat lowered the temperature, because it has been accompanied by a corresponding removal of mass from the interior at the internal temperature without correspondingly diminishing the volume. Its expansion into steam having taken place after it has entered the region of the crust will not have directly affected the temperature of the interior. On the other hand it must be borne in mind that sources exist from which the heat of the interior is continually recruited; namely that arising from the gravitation of the shrinking mass towards the centre, and that from the friction of the bodily tides[1].

[1] See Darwin, "On the Precession of a viscous Spheroid and the remote history of the Earth." "Phil. Trans.," Pt. II., 1879, pp. 494, 532.

SUMMARY OF APPENDIX.

XXVI. "*The suboceanic crust,*" *further considerations*—XXVII. *Thickness of the crust and the world's age*—XXVIII. *On the cooling of an earth not solid.*

XXVI. Observations upon the value of gravity taken with the pendulum upon islands, after allowance has been made for the peculiar circumstances of such stations, prove that the surface of the ocean would be spherical were it not for the effects of the earth's rotation. The arrangement of the materials must therefore be such that, setting aside the rotation, the gravitational effect would of itself produce equality of attraction over the surface of the ocean. We have in the XVIIth chapter obtained a mathematical expression for the determination of such an arrangement. In order to render our investigation applicable to the circumstances, we have supposed the earth to consist of a spherical nucleus, whether liquid or solid, made up of concentric layers, and above that of a crust and substratum with the addition of the mass of the ocean over the aqueous portions. We then regard the crust as formed of layers, differing in number, density and thickness at different localities. Since however our formulæ apply only to those parts of the surface which are truly spherical, they exclude such areas of the land as are not sensibly nearly at the ocean level.

The complexity of the formulæ leads us to simplify them by reducing the consideration of the crust to the case of not more than two layers and a substratum, with the addition of the oceanic envelope over the aqueous portions. This is a generalisation of what the condition of the crust must be, when considered throughout its whole thickness at any place; for it must either be of uniform mean density, or else it must be on the average more dense in the upper than in the lower portion, or else less dense in the upper than in the lower portion; and if

either of the latter, then the upper or lower portion may be the thicker or thinner of the two. Our equations enable us to discover what these relations as to density and thickness are.

An interesting letter from Mr Jukes-Browne, which appeared in "Nature[1]," has led us to reconsider the interpretation of the equations; and we have found that an assumption which was made in the "Summary," although a very natural one to make, was not justified. It was that, on the hypothesis of the crust beneath the ocean consisting of two layers, the lower one would be more dense than the upper. We find that a fuller examination of the equations shows that the arrangement of the densities must be the reverse of this, and that beneath the ocean the upper layer is more dense than the lower; and, when this is taken account of in the formulæ, it alters some of the other conclusions in important particulars. The reader is therefore requested to substitute what follows for the corresponding paragraphs on pp. 364, 365.

(1) The suboceanic crust dips less deeply into the substratum than does the continental crust at the seaboard.

(2) The suboceanic crust is less dense in the lower than in the upper portion.

(3) The lower portion of that crust is also less dense than the substratum beneath it.

(4) The upper layer of the suboceanic crust is of a high density, and quite thin when compared to the whole thickness.

(5) The substratum beneath the ocean is less dense than beneath the seaboard.

(6) The continental crust at the seaboard is of uniform density throughout, or, if it does consist of two layers of different density, one of them must be too thin to sensibly affect the gravitational phenomena.

The above conclusions depend upon the assumption that the continental crust is less dense than the substratum on which it

[1] Nov. 21, 1889, vol. XLI., p. 54.

rests, together with a few others obviously true, as for instance that rock is more than twice as dense as water.

It follows from (1) that the suboceanic crust is not so thick as the continental crust at the seaboard; and it appears that, if we take 25 miles for the thickness of the latter, that of the former must be less than 20 miles; and we find that, if we assume it to be 18 miles where the depth of water is 5 miles, this, with the hitherto assumed values of the densities of the continental crust and its substratum, viz. 2·68 and 2·96, brings out the density of the thin upper layer beneath the ocean as 3, which, seeing that we require for it as high a value as we can fairly assign to rock, seems suitable.

These conclusions will equally apply in principle if we accept the value for a thinner crust as given in the XXVIIth chapter, which we have obtained on the basis of new determinations of the melting temperature and latent heat of basalt.

The physical possibility of these results is then examined.

If the substratum is liquid, the fact that it is less dense beneath the oceans would produce upward convection currents beneath those regions. This would make the supply of heat to be more rapid there, and cause the crust to thicken more slowly. The two results, (1) and (5), are therefore in harmony, and we have already seen on other grounds, in the VIth and XXVIIth chapters, that convection currents must exist. The greater thinness of the crust beneath the ocean is thus accounted for.

It will follow that the temperature gradient must be greater beneath the ocean, and the flow of heat through the crust more rapid; and these effects will be intensified by the low temperature at the bottom of the deep oceans, which will give rise to a greater difference between the extreme temperatures within a shorter distance.

It appears that, where the water becomes less deep, the crust becomes thicker; but we cannot apply our formulæ satisfactorily to regions near the shore-line, where we cannot assert that the crust belongs exclusively either to the suboceanic or to the continental type, having probably been often alternately submerged and reelevated.

As regards the bearing of the preceding modified results

upon dynamical geology, it will be seen that the conclusion at which we had already arrived in Chapter XVII., respecting the respective densities of the substratum beneath the oceanic area and the seaboard, is confirmed, and the existence of upward currents established beneath the ocean. Downward return currents must therefore be situated somewhere beneath the continents. All the arguments which rest upon this conclusion will therefore retain their force, and what has been advanced in section XXIII. of the "Summary" will not only hold good, but will be strengthened. For the surface flow of the substratum from the oceanic areas toward the land will work at a greater advantage to produce compression, if the suboceanic crust does not dip so deeply into it as the continental crust, contrary to what we had been at first led to suppose.

It is evident that whatever amount of compression we may attribute to this cause presupposes a correlative extension of the suboceanic crust. This has probably been accompanied by the rending of fissures filled with lava, which will have constantly led to overflows of dense rock on the floor of the ocean. It is possible that the thin and very dense upper stratum, the existence of which needs explanation, may be owing to this cause, partially at any rate; although it is conceivable that, if our speculation in Chapter XXV. about the origin of ocean basins be correct, it may consist in part of the dense magma suddenly cooled, before the basic minerals had time to crystallize out and subside.

XXVII. In the sixth chapter we made use of values for the melting temperature of rock deduced from that of slag, according to determinations made by Mallet, and recorded in his paper on Volcanic Energy[1]; and we assumed the latent heat of rock to lie between what must be extreme limits. With the help of these values we obtained certain estimates for the world's age upon the hypothesis that the thickness of the crust is 25 miles. Professors Rücker and Roberts-Austen have now examined a natural rock, and obtained good results.

The experiments were undertaken at the suggestion of Professor Judd. The rock examined was the basalt, or dolerite,

[1] "Phil. Trans.," vol. CLXIII., 1873.

of Rowley Regis. It was found that it began to soften at 760° C. (1400° F.), and that fusion was complete at 920° C. (1688° F.). The mean specific heat was about 0·27, and the whole amount of heat taken up in fusing unit mass of rock was about 49·60 times the amount of heat required to raise unit mass of water through one degree centigrade. It will be recollected that the latent heat of water corresponds to the definite temperature 0° C. or 32° F., whereas that of the rock experimented on is as it were spread over a range of 160° C., extending from 760°, when it begins to soften, to 920°, when it becomes quite liquid.

The temperature 1688° F. of complete fusion is considerably lower than Mallet's value for slag, and, seeing that his slags were slightly more basic than the basalt and therefore probably more fusible, there can be no doubt that his estimate was too high. Possessed of these newly determined values of the melting temperature and latent heat, we are in a better position than before for forming some estimates of the thickness of the crust and the world's age, as well as regarding the action of the substratum. We have therefore taken for the melting temperature 1688° F., and for the latent heat 406° F., the conductivity of the crust, defined as heretofore, at 400, while the temperature gradient at the surface is one degree F. in 51 feet. We have then shown that on the hypothesis that the crust is in process of being formed out of a still liquid, free from convection currents, the thickness of the crust and the world's age are determinate quantities, which admit of calculation, exact so far as the above quantities can be depended on as correct.

We should in that case have for the thickness of the crust about 22 miles, and for the world's age rather more than 8 million years.

Now 8 million years is a far shorter period than geological observations seem to require for the changes in physical and organic conditions upon the earth since the crust became solid; and consequently our newly acquired knowledge of the phenomena attending the melting of basalt fully confirms the conclusion we had already arrived at, when we were able to use only tentative values for the melting temperature and

latent heat; namely the conclusion that the substratum cannot be an inert liquid.

If the substratum is not an inert liquid, but exercises an influence in delaying the thickening of the crust, it seems that the only mode in which this can be effected is by the action of convection currents. We must therefore assume their existence without professing to be able fully to account for them. No doubt a mass of cooling liquid would be affected with convection currents; but the evolution of gas from a liquid would also have a similar effect. Both these causes may probably co-operate.

Since then we cannot believe the substratum to consist of an inert liquid, we are obliged to make an assumption concerning one of the three quantities which are not known, and then to find what values of the others will agree with it. The three quantities which we do not know are, (1) the thickness of the crust, (2) the age of the world, (3) the amount of activity in the substratum. In our former estimates[1], owing to our ignorance of the melting temperature and latent heat, we found it convenient to assume the thickness of the crust to be 25 miles, and to use not improbable values of the latent heat. But now it seems better to assume values for the age of the world, and to calculate the corresponding values for the thickness of the crust and the activity of the substratum. Two periods have accordingly been assumed for the world's age. These are respectively 100 million years, and 50 million years, the longer period being most in accordance with geological opinion. We find the preliminary result that, if the world's age is over 50 million years, the thickness of the crust at the present time cannot differ much from 17 miles. If it were greater, the temperature gradient at the surface would be now smaller than 1° F. for 51 feet of descent.

Supposing then that the age of the world is 100 million years, our calculations give the present thickness of the crust to be 16·66 miles, and the present rate of thickening 4 feet in 10,000 years, and the retardation of the thickening 18 feet in 1000 years.

[1] Chap. VI., p. 75.

But if we suppose the age of the world to be only 50 million years, then the thickness of the crust would be 16·84 miles, which is a little greater than if the time is taken as 100 million years. The present rate of thickening would be about 9 feet in 10,000 years, and the retardation of thickening about 16 feet in 1000 years.

The reason why the crust would be somewhat thicker when the time is shorter than when it is longer, the temperature rate at the surface being the same in both cases, is on account of the much higher rate of retardation of thickening required in the case of the longer period.

The experiments made upon the basalt render it probable that the part of the crust beneath, which is more or less softened by heat, or rather which has not yet completely hardened, may include nearly a quarter of the whole thickness. Making allowance for this circumstance we ought perhaps to reckon a rather greater thickness for it than our calculations give.

The temperature 1688° F. obtained by experiment for the complete melting point of basalt, as likewise the value of about 17 miles for the thickness of the crust, agree fairly well with the corresponding quantities which we formerly obtained[1] from considerations of densities and of temperature gradients at the sea level and at Mt S. Gothard and at Mt Cenis. This circumstance affords some corroboration of the correctness of all the data involved.

XXVIII. In the eighth chapter we discussed the results of the cooling of the earth supposing it solid throughout, and we found the depth of the level of no strain, above which the rocks would be undergoing compression and below it extension at the present time. Taking the melting temperature at 4000° F., this level of no strain would in that case be situated at the depth of less than a mile, and the mean height of the elevations which would have resulted from the compression would not be more than a few inches. But, as stated in the summary of that chapter, "if the crust reposes on a liquid substratum, the layers added to it from beneath will have been of the fitting size at

[1] Chap. XVI., pp. 227, 228.

the time of their addition, and the conditions of the problem of contraction would be greatly altered[1]."

We have accordingly now investigated the problem under these conditions, which we believe to represent the case of nature. We begin with some considerations respecting the physical conditions of the liquid substratum. We have seen good reason to believe that it is affected with convection currents, although we cannot fully know the causes which produce them. It is not probable that such currents would pass between liquids of different kinds, as for instance between molten rock and molten metal. We have therefore enquired what the probable constitution of the interior may be in this respect, so that the density may follow nearly Laplace's law; and it appears that a magma similar to melted basalt may possibly occupy a thickness of a few hundred miles beneath the crust, but that lower down materials of greater intrinsic density must replace it. In the next place we have gone more fully into an examination of the argument for solidity derived from the supposed existence of a fortnightly tide, with the result of finding that observations hitherto made cannot be relied on to establish the existence of such a tide.

These preliminary questions being disposed of, we recapitulate the definition of a level of no strain, and set about to calculate its position under the conditions of a liquid interior, using the values for the melting temperature and latent heat lately obtained by Professors Rücker and Roberts-Austen. But this calculation involves still another constant, for determining which we have no certain data, that is, the coefficient of contraction of the interior. Nevertheless some experiments were formerly undertaken by Mallet, which appear to bear upon this question; for Mallet measured the contraction of slags while in a state of fusion as well as after solidification. He found their contraction to be about twice as great before as it was after solidification.

Since the level of no strain sinks lower and lower as time goes on, its position will depend upon the age of the world. We have assumed for this the same two values as in the preceding

[1] p. 354.

chapter, namely 100 million years and 50 million years; and we then find that if the period has been 100 million years, the depth of the level of no strain would be at the present time about 4 miles, and the mean height of all the elevations, due to the corresponding corrugation of the matter above that level owing to secular cooling, would be about 44 feet: and under the same circumstances the total radial contraction would be 12 miles. If however the time was only 50 million years, the depth of the level of no strain would be 2 miles, and the mean height of the elevations formed would be a little over 10 feet. In that case the radial contraction would have been 6 miles. These values would all have to be somewhat increased if the coefficient of contraction in the interior was taken larger; for instance, if it was 3 instead of 2, for 100 million years the mean height of the elevations would be 97 instead of 44 feet. Under the same circumstances the radial contraction also would be greater, being 18 miles for 100 million years, and $8\frac{1}{2}$ miles for 50 million years.

It appears therefore as the result of the investigations in this chapter, that the hypothesis of a liquid substratum does not afford such an increased amount of compression, as to render it possible to attribute the elevation of mountains to contraction through cooling in that case, any more than in the case of solidity.

We are enabled likewise to form an estimate of the total mean fall of temperature of the interior since the time when a crust began to be formed. If the time elapsed has been 100 million years, the mean fall would have been about 209° F., so that in early times the globe as a whole will not have been much hotter than it is at present. This estimate does not require a knowledge of the past or present temperature of the deep seated strata, but only of the melting temperature of crust rock. If therefore volcanic energy was much more active in early geological times than it is at present, the reason must be looked for rather in a thinner crust and a greater quantity of occluded gas than in a much hotter nucleus.

INDEX TO THE APPENDIX.

[The numbers printed in brackets refer to the Summary.]

CAMBRIDGE: PRINTED BY C. J. CLAY, M.A. AND SONS, AT THE UNIVERSITY PRESS.